TRANSITS OF VENUS.

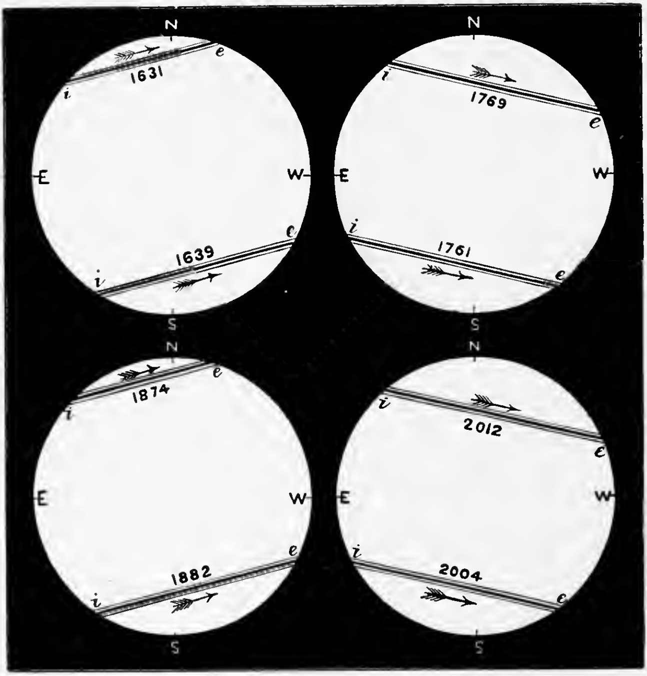
LONDON: PRINTED BY
SPOTTISWOODE AND CO., NEW-STREET SQUARE
AND PARLIAMENT STREET

PLATE I.

PATHS OF VENUS

(MOST NORTHERLY, CENTRAL, AND MOST SOUTHERLY)

ACROSS THE SUN'S FACE



R. A. Proctor del.

DURING THE TRANSITS OF

A.D. 1631, 1639, 1761, 1769, 1874, 1882, 2004, AND 2012.

(The regions where the ingress *i*, the egress *e*, or the whole transit could be seen, are shown in the eight coloured plates II.—IX.)

TRANSITS OF VENUS.

A POPULAR ACCOUNT
OF
PAST AND COMING TRANSITS

FROM THE FIRST OBSERVED BY HORROCKS A.D. 1639
TO THE TRANSIT OF A.D. 2012.

BY

RICHARD A. PROCTOR,

AUTHOR OF 'SATURN' 'THE SUN' 'THE MOON' 'OTHER WORLDS THAN OURS'
'THE UNIVERSE' ETC.

> A spot like which
> Astronomer in the Sun's lucent orb
> Through his glazed optic tube yet never saw—MILTON.
>
> Et vera incessu patuit Dea—VIRGIL.

WITH 20 PLATES (12 COLOURED) AND 38 WOODCUTS.

ALSO AN ACCOUNT OF THE SUCCESSES ACHIEVED IN DECEMBER 1874
AND SUGGESTIONS RESPECTING THE TRANSIT OF 1882.

21963
13/2/1892

FOURTH EDITION.

LONDON:
LONGMANS, GREEN, AND CO.
1882.

PREFACE

TO

THE FOURTH EDITION.

THE approach of the transit of Venus on December 6 (partly visible in this country), and very favourably visible throughout its entire duration in the United States, renders necessary a new edition of this work. Very few changes have been made, as the second and third editions both followed the transit of 1874, and very little has been done in the interval which renders change necessary or desirable. The estimates now regarded as probably nearest the truth assign to the sun a distance of about 92,885,000 miles, corresponding to a mean equatorial long horizontal solar parallax of 8″80. It remains to be seen whether observations made on the approaching transit will modify this value.

RICHARD A. PROCTOR

LONDON: *June* 1882.

PREFACE

TO

THE SECOND EDITION.

THOUGH I have had good reason for believing that during the last few years there has been a remarkable increase of interest in scientific subjects, I do not know that any circumstance has tended more directly to convince me of this than the welcome extended to the present volume. It was justly remarked, in a review which appeared in the 'Quarterly Journal of Science,' that 'the work' could hardly appeal to so large a section of the public as the 'Sun,' 'Moon,' &c., 'for the subject is one that rather concerns the astronomer than the general public; the objects at issue do not bear so distinctly upon every-day life; and if a man has never seen Venus, and has no chance of ever seeing a transit, and does not easily comprehend how his race can be benefited by an exact determination of the sun's distance, he is not likely to trouble himself about the subject at all.' The sale of 1500 copies of the present work in six months shows that many besides astronomers have taken interest in the efforts made during the recent transit to improve our knowledge of the dimensions of the solar system.

In the present edition I have given a general account of the successes achieved last December. I have also directed special attention to the advantages which may attend the use of the mid-transit method in 1882. See pp. 226–229. Fig. 44, p. 228, shows what regions would require to be occupied for this purpose.

A few passages in the former edition, correcting statements in which the controversy pending the late transit had been erroneously described, have been removed. I am glad to be able to say that since the present work appeared matters have been in general very fairly represented; and it may well be hoped that any soreness still remaining in certain quarters will before long disappear altogether.

RICHARD A. PROCTOR.

LONDON: *May* 1875.

PREFACE

TO

THE FIRST EDITION.

THIS WORK is intended to be partly historical and partly explanatory. So far as I know, no book has hitherto been published in England giving a complete account of the transits of 1639, 1761, and 1769, and of the various interesting circumstances connected with them. This want I have endeavoured here to meet, illustrating by maps the conditions under which those transits were observed. In the chapters relating to the transits of 1761 and 1769, I sketch the causes of the partial failure of the observations then made, and give an account of the attempts made in recent years to reconcile those observations with the present estimate of the sun's distance. It will be observed that in dealing with the latest of these attempts I adopt the opinion of Continental and American astronomers, no longer regarding that attempt as in any sense removing the difficulties recognised before it was made.

In Chapter IV. I have given a simple account of the principles on which the recurrence and observation of transits depend.

In the last chapter I carry on the history of the subject to the present time. It would be impossible, as Sir Edmund Beckett points out in the latest edition of his fine work 'Astronomy without Mathematics,' to present the subject adequately without a short account of the occurrences of 1869 and 1873—now belonging to the history of transits, and instructive in many respects. It has seemed to me best to quote the original papers of 1868 and 1869, and then briefly sketch the progress of events which led to the arrangements finally adopted.

A brief account is given at the end of Chapter V. of the conditions of the transits of 2004 and 2012.

RICHARD A. PROCTOR.

LONDON: *October* 1874.

For the use of Plates X., XI., XII., XIII., and XIV., I have to thank the Editor of the 'Astronomical Register,' Rev. S. J. Jackson. These pictures originally appeared in the 'Illustrated London News,' for which journal I drew them. Electros were supplied to Mr. Jackson for use in the 'Register,' and were lent to me by him. These plates have also appeared, slightly reduced by some process unknown to me, in the New York 'Daily Graphic.'

CONTENTS.

ILLUSTRATIONS.

PLATES.

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[1] Plates II.—IX., and Plates XVII., XVIII., are to be arranged so that all the titles, TRANSIT OF 1631, 1639, &c., lie towards the left when the book is opened between any pair of plates and held in the usual manner. Plate XX. is to read the other way; that is, it is to have its title, Plate XX., towards the right.

WOODCUTS IN THE TEXT.

TRANSITS OF VENUS.

CHAPTER I.

TRANSITS OF THE SEVENTEENTH CENTURY.

As soon as the Copernican theory of the solar system was established, astronomers perceived that the inferior planets, Mercury and Venus, must from time to time appear to cross the face of the sun. For although on most of these occasions when either planet passes between the sun and the earth, no transit was to be expected, the planet either passing above or below the face of the sun, yet it could not but happen that, in the course of many such conjunctions, the planet would make a passage at so small a distance north or south of the sun's centre as to appear for a time to be upon the sun's disc. To make this clear, without entering into any nice details at this stage, let E *e* and V *v* (fig. 1) be the paths of the Earth and Venus, respectively, around the Sun, S.[1] Then if we suppose, the path E *e* to lie in the level of the paper, we must imagine one-half of the path V *v*—the half V′ *v v*′—to lie above the level of the paper, the other half, *v* V V′, lying below that level—not greatly above or below; in fact, the short white lines near *v* and V show how much these parts of the path of Venus are to be supposed respectively above and below the level of the paper. Still, it will be clear that when Venus is, as at V, between the Sun and the Earth at S and E, she is not really on the line S E, but considerably below it; and, as supposed to be seen from the Earth, she passes below the Sun. When in conjunction on the other side of V′ *v*′, Venus passes above the Sun. Only when she is in conjunction nearly at V′ (the Earth near E′), or nearly at *v*′ (the Earth near *e*′), will she

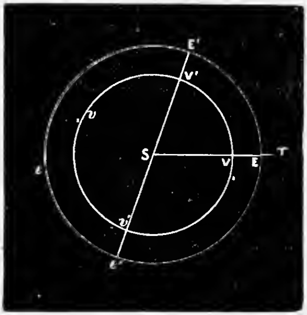

Fig. 1.—Showing the paths of Venus and the Earth, and indicating their inclination and the line of intersection of their planes.

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[1] E is supposed to be the place occupied by the earth at the time of the autumnal equinox.

appear to cross the face of the Sun. But in the course of many conjunctions there must be some which take place when the two planets are thus placed; that is, either near V′ and E′, or near *v*′ and *e*′. Similar remarks apply to the case of Mercury.

This was early perceived by the followers of Copernicus. The Ptolemaic system did not, indeed, preclude the possibility of such phenomena as transits; but since the older astronomers regarded the planets as shining by their own inherent lustre, it was not to be expected that, even if a transit occurred, the planet would be discernible while crossing the sun's face. And as in reality the Ptolemaic system gave no means of inferring the relative distances of the several planets (including the sun[1]), there could not even be any certain assurance that Venus or Mercury ever came between the earth and the sun. It was only by a mere assumption that the old astronomy assigned to the sun a sphere outside the spheres of Mercury and Venus.

Still we find that, even so far back as the ninth century, Mercury was supposed to have been seen as a dark spot on the face of the sun. Doubtless one of those large sun-spots, which from time to time are visible to the naked eye, had attracted attention, and was regarded by the ignorant as caused by the passage of one or other of the planets, Mercury or Venus, between the earth and the sun. Venus being probably

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[1] The sun and moon were 'planets' in the old astronomy; and we still find traces of the usage in some modern expressions.

conspicuous at the time, either as a morning or evening star, the less familiar planet Mercury afforded a convenient explanation of the dark spot. The account of the phenomenon accords well with the belief that a solar spot was seen, for we are told by the author of the 'Life of Charlemagne' that Mercury was visible as a black spot upon the sun for eight consecutive days in April of the year 807. Kepler, who was perfectly well aware that Mercury moves too rapidly to remain even for as many hours on the sun's disc, endeavoured to show that the expression originally used in the manuscript had not been *octo dies*, but *octoties*, a barbaric form of *octies*, for 'eight times.'

It is now well known that Mercury is far too small to be seen by the naked eye when crossing the sun's disc. And this fact disposes of the statement made by the famous physician Ebn Roschd (commonly called Averroës), in his Ptolemaic Paraphrase, to the effect that he saw the planet on the sun in the year 1161, at a time when Mercury really was in inferior conjunction. We need not, however, question the veracity of the learned doctor, seeing that Kepler himself supposed he had seen the planet upon the sun on one occasion. When, a few years later, the existence of sun-spots was detected by the telescope, Kepler admitted that in all probability he had seen such a spot, and not the planet Mercury.

After Kepler had completed his Rudolphine tables ot the planetary motions he was able to arrive at tolerably accurate results as to the epochs of the transits of Mercury and Venus over the solar disc. In fact, he announced, in 1627, that in the year 1631 both Mercury and Venus would pass over the sun's face—Mercury on November 7, and Venus on December 6; and that in 1761 Venus would again pass across the face of the sun.

As the first occasion on which the transit of an inferior planet was ever witnessed, the transit of Mercury in 1631 has an interest resembling that which attaches to the first observation of a transit of Venus, eight years later. Therefore I think the reader will be interested to hear how Gassendi succeeded in observing Mercury in transit.

Gassendi made preparations for the observation of the transit at Paris. The manner in which he observed the phenomenon was somewhat remarkable. Through a small aperture in a shutter the solar light was admitted into a darkened room, and an image of the sun, some nine or ten inches in diameter, was formed upon a white screen. A carefully divided circle was traced upon this screen, and the whole was so arranged that the image of the sun could be made to coincide exactly with the circle. As Gassendi was anxious to ascertain the exact moment of the ingress of the planet upon the sun's disc, or—supposing he should fail in that respect—at least to determine the moment of egress, and as he had no trustworthy clock, he determined that the altitude of the sun should be carefully estimated several times during the progress of the transit, and particularly at the moment of egress. It was necessary, therefore, that he should have an assistant, and, further, that his assistant should work in another room; for from the room in which Gassendi was working the sun's light, as I have said, had been carefully excluded, save at the minute aperture in the window-shutter. Accordingly, Gassendi placed his assistant in a room above him, with a large quadrant for taking altitudes, instructing him to observe the height of the sun as soon as he heard Gassendi stamp upon the floor of the room beneath. A clumsy arrangement, truly, when compared with the subtle devices of modern astronomers—with the aid which they derive from powerful telescopes, all but perfect clocks, and, where need arises for communicating with one another from distant stations, the instantaneous indications of telegraphy. Yet we cannot but admire the spirit in which Gassendi worked, the readiness with which, for want of more perfect instruments, he set himself to invent arrangements which suited his requirements, and the skill with which he availed himself of those imperfect adaptations.

And if we admire these qualities in Gassendi, still more must we admire the patience with which he waited for the commencement of the phenomenon. Modern astronomy is able to announce, within three or four minutes, the instant at which a transit will commence at any given spot upon the earth's surface. But Kepler's prediction respecting Mercury's motions did not lay claim to any accuracy of this sort. So uncertain did the epoch of the occurrence appear to be, that Gassendi began to watch for the transit *two days* before the date assigned by Kepler for its occurrence.

The 5th of November proved unfavourable for observation, the day being rainy. The next day was also unsuitable, clouds having overspread the sky during nearly the whole day. The morning of the 7th, the day appointed by Kepler for the transit, was also cloudy. Thus Gassendi began his watch on that day with the uncomfortable feeling that during some part of the two preceding days the planet might already have passed over the sun's disc,—perhaps that the transit had been completed but a few minutes before the clouds broke up on the morning of the 7th.

A little before eight the sun shone for a few minutes through the openings between the clouds, but there still remained enough mist to prevent Gassendi from being able to determine whether any spot existed upon the image of the sun in his observing-room. Nearly an hour passed before the sun was sufficiently clear of clouds to enable Gassendi to make any satisfactory observations. Towards nine, however, the sun became distinctly visible, and turning to the image on the screen, the astronomer perceived upon it a small black spot. He could not believe, however, that this was Mercury, as the received estimate of the planet's dimensions had led him to look for a spot nearly twice as large. As he was familiar with the nature of solar spots, and the rapid manner in which they form, he concluded that one had made its appearance on the sun's surface since the preceding day. At nine o'clock he had another opportunity of observing the spot, and he carefully estimated its position, intending to make use of it as a point of reference for determining the path of the planet in transit, if he should be fortunate enough to witness that phenomenon. Soon after, he had another view of the spot, and was surprised to find that it had moved away considerably from its former position. He felt assured that no ordinary solar spot could have moved so rapidly; but still he could not persuade himself that he was looking at Mercury in transit, having so fully satisfied his mind respecting the dimensions which the planet would exhibit. Besides, the hour had not yet arrived at which Kepler had predicted that the transit would begin.

Gassendi was still in doubt, and endeavouring to recall the circumstances of his former measurement, in order to convince himself that he had made no mistake, when the sun again made his appearance through the clouds, and it was apparent that the spot had moved yet farther from its original place. No room now remained for doubt. It was clear that the phenomenon which had been so long and so anxiously awaited by the astronomer was already in progress. He immediately stamped upon the floor to attract the notice of his assistant. But this person, whose name has not reached us, was possessed of less patience than Gassendi. He probably felt much less interest in the phenomenon; possibly, he placed very little faith in the calculations of Kepler. Whatever was the reason, he had grown weary of watching, and had left his post. Gassendi had to continue his observations alone, hoping that at least his assistant would return before the planet had passed completely off the sun's face. Fortunately this happened; the requisite observations were made for determining the time of egress; and thus an important addition was made to our knowledge of the motions of the innermost planet of the solar system.

Gassendi sent an amusing account of his observations to Professor Shickhard, of the University of Tübingen. 'The crafty god,' he wrote, 'had sought to deceive astronomers by passing over the sun a little earlier than was expected, and had drawn a veil of dark clouds over the earth in order to make his escape more effectual. But Apollo, acquainted with his knavish tricks from his infancy, would not allow him to pass altogether unnoticed. To be brief, I have been more fortunate than those hunters after Mercury who sought the cunning god in the sun. I found him out, and saw him where no one else had hitherto seen him.' He states that the planet, as seen projected on the image of the sun, did not appear altogether black, but was greyish, and somewhat ruddy round the margin. Doubtless these peculiarities were due to the method of observation employed by the astronomer. He estimated the apparent diameter of the spot at about one-ninetieth part of the sun's apparent diameter, an estimate considerably exceeding the true dimensions, but still more considerably below the dimensions which astronomers had been disposed to assign to the planet.

Gassendi, although he did not observe the commencement of the transit, was yet able to compute the time of its occurrence. He found that the transit had begun nearly five hours before the time assigned by Kepler.[1]

I have mentioned that Kepler had predicted that a transit of Venus would take place on December 6, 1631. It need hardly be said that Gassendi, after his

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[1] The second observed transit of Mercury took place on November 3, 1651. The observations of Gassendi had enabled astronomers to estimate the epoch of the transit much more exactly than in the former instance. It resulted from their calculations that the phenomenon would not be visible in England, or indeed in Europe; but would be well seen over a large part of Asia. Accordingly a young Englishman, Jeremiah Shakerley, went to Surat, in India, for the purpose of witnessing the phenomenon. Such a journey undertaken for such a purpose in an age when sea-voyages were not only much more protracted, but also far more dangerous than in the present day, must be looked upon as a remarkable and commendable instance of devotion to scientific pursuits. It is pleasing to be able to record that the energy of the young Englishman was rewarded by complete success.

The third observed transit took place on May 3, 1661. It was observed by Hevelius at Dantzic, and at London by Huyghens, Street, and Mercator. Hevelius was surprised to find that the diameter of the planet was very much smaller than he had been led to expect. He found on measurement that Gassendi's estimate was nearly twice as great as the true diameter of the planet.

The fourth transit of Mercury was observed by Halley at St. Helena on November 7, 1677. He was the first astronomer who had ever observed the complete passage of the planet across the solar disc.

Later transits of Mercury have no special historical interest, though observations of considerable importance were made during the transits of 1736, 1799, and 1868.

success in observing the transit of Mercury, had good hopes of observing Venus to even greater advantage. It is true that, according to Kepler's calculation, the transit might be expected to begin only towards sunset, and it was therefore possible that the phenomenon would not be visible at all in Europe. But it was equally possible that any error in the calculation might lie in the other direction, and so the whole transit be favourably seen before sunset. Gassendi took the same measures for observing the transit as in the case of Mercury. He had proposed to observe the sun on December 4 and 5, but 'an impetuous storm of wind and rain rendered the face of the heavens invisible on both those days. On the 6th he continued to obtain occasional glimpses of the sun till a little past three o'clock in the afternoon, but no indication of the planet could be discerned upon the sun's disc as depicted upon the white circle. On the 7th he saw the sun during the whole forenoon, but he looked in vain for any trace of the planet.' 'It is now well known,' proceeds Prof. Grant, from whose account I have just quoted, 'that the transit of the planet took place during the night between December 6 and 7.' I do not know where any calculation of the circumstances of the transit can be found; but an investigation of my own (sufficiently accurate for a past and unseen phenomenon) shows that in the South-eastern parts of Europe the egress might have been observed, occurring for those parts after sunrise on the morning of December 7.[1] Plate II. shows the parts of the earth whence the transit might have been seen wholly or in part. The position of the line C D, which, according to my calculation, marks the boundary between the places where day and night were in progress in the Northern hemisphere at egress, shows the parts of Europe whence the end of the transit might have been observed.

Kepler had stated that after the transit of 1631 there would be none till the year 1761. According to his calculations, Venus, when in her inferior conjunction on December 4, 1639, would pass very near to the sun's centre, but not quite near enough for a transit to occur, the planet passing *below*—that is, south of the sun. On the other hand, the tables of Lansberg, a Belgian astronomer, who followed the old system of computation, seemed to show that Venus would on this occasion transit the upper or northern part of the sun's face. Horrocks, a young and then unknown astronomer, having been led to examine the tables of Venus, found that though Kepler's were much more exact than Lansberg's, a transit would really occur, Venus passing below the centre of the sun, as Kepler had predicted, but not so low as to miss the

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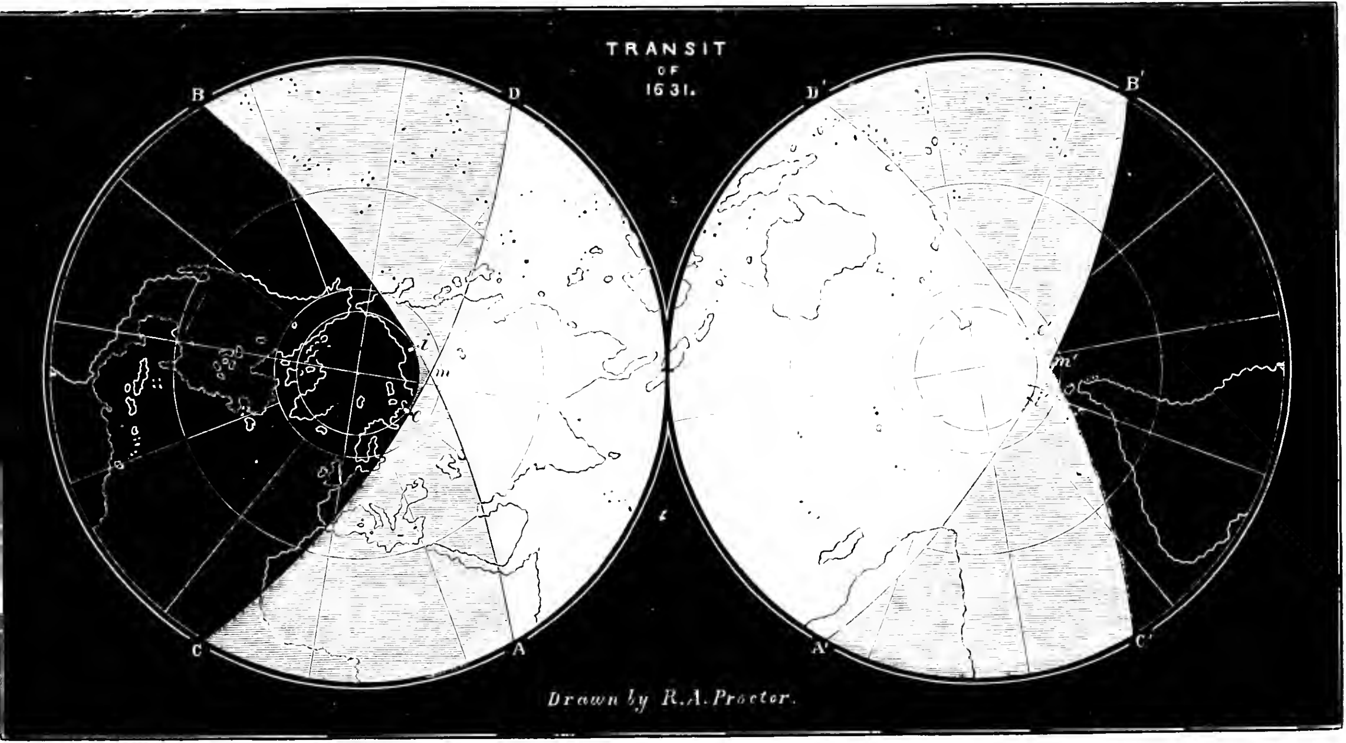
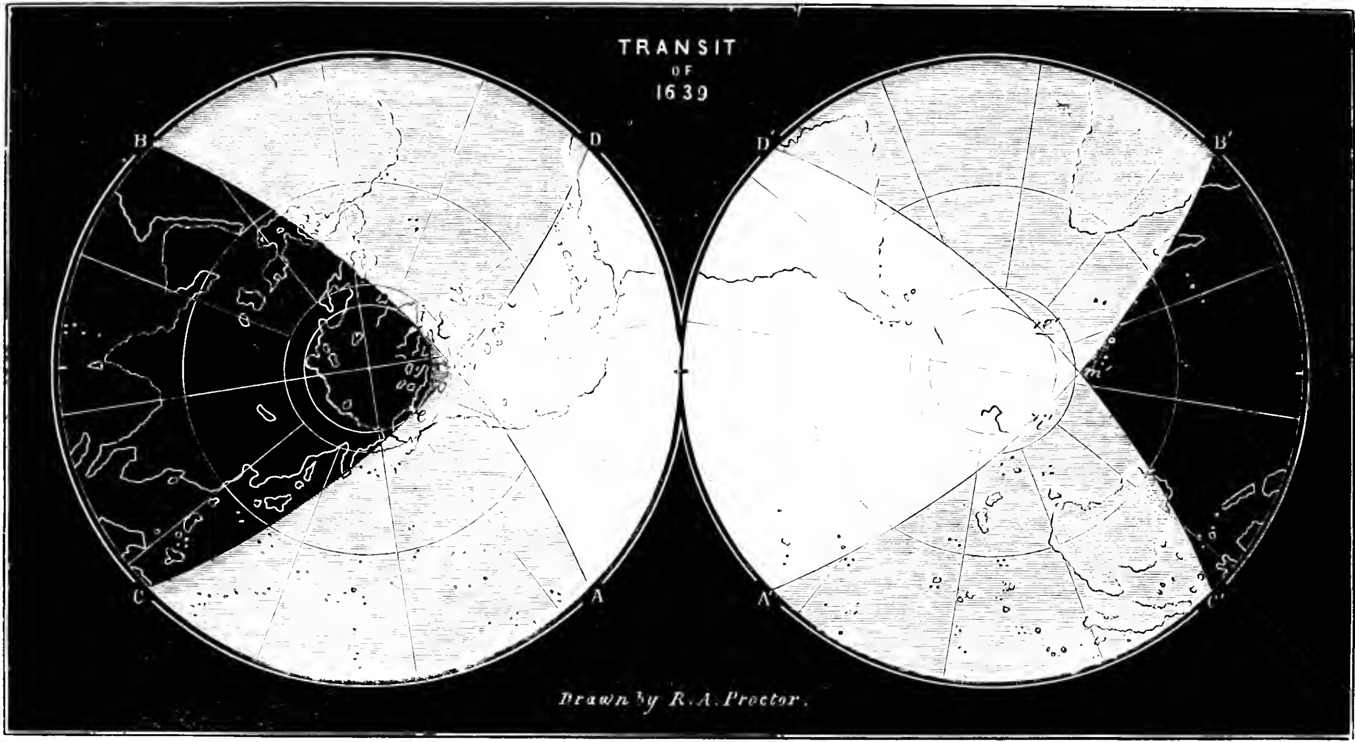
[1] M. Dubois, in his admirable work, 'Les Passages de Vénus,' gives the following humorous explanation of Gassendi's failure:—'Le Passage de Vénus, qui sans doute n'était pas prédit avec une précision suffisante, ne fut pas observé—*d'abord* parce que Gassendi, qui s'apprêtait à l'observation, en fut empêché par la pluie, mais *surtout* parce que le passage eut lieu pendant la nuit pour les observateurs européens.' [The italics are mine.]

PLATE II.



A B and A′ B′ mark the boundary between the sunlit and dark hemispheres at the beginning of the transit.
C D and C′ D′ mark the boundary between the sunlit and dark hemispheres at the end of the transit.



A B and A′ B′ mark the boundary between the sunlit and dark hemispheres at the beginning of the transit.
C D and C′ D′ mark the boundary between the sunlit and dark hemispheres at the end of the transit.

sun's disc altogether. The circumstances under which Horrocks made this discovery possess considerable interest, and I propose to devote some space to his account of them.

While preparing himself for practical observation, Horrocks undertook (apparently from sheer love of science) the computation of Venus's motions from the tables of Lansberg. These tables were so highly valued by their author that he had spoken of them as superior to all others, *quantum lenta solent inter viburna cupressi.* But Horrocks recognised many imperfections in them, and at length, as he says, 'broke off the useless computation, resolved for the future with his own eyes to observe the positions of the stars in the heavens; but, lest so many hours should be entirely thrown away,' he made use of his results to predict the positions of the planets. 'While thus engaged, I received,' he proceeds, 'my first intimation of the remarkable conjunction of Venus and the sun; and I regard it as a very fortunate occurrence, inasmuch as about the beginning of October it induced me, in expectation of so grand a spectacle, to observe with increased attention.' Nevertheless, his heart was wroth within him against Lansberg, insomuch that he could not refrain from the extreme step of 'forgiving' him in the following agreeable terms: 'I pardon, in the meantime, the miserable arrogance of the Belgian astronomer who has overloaded his useless tables with such unmerited praise, and cease to lament the misapplication of my own time, deeming it a sufficient reward that I was thereby led to consider and to foresee the appearance of Venus in the sun. But, on the other hand, may Lansberg forgive me' (this is charming) 'that I hesitated to trust him in an observation of such importance, and from having been so often deceived by his pretensions to universal accuracy that I disregarded the general reception of his tables.' 'Lest a vain exultation should deceive me,' he proceeds, 'and to prevent the chance of disappointment, I not only determined diligently to watch the important spectacle myself, but exhorted others whom I knew to be fond of astronomy to follow my example; in order that the testimony of several persons, if it should so happen, might the more effectually promote the attainment of truth, and because by observing in different places our purpose would be less likely to be defeated by the accidental interposition of clouds, or any fortuitous impediment.' He was particularly anxious, because Jupiter and Mercury seemed by their positions to threaten bad weather. 'For,' says he, 'in such apprehension I coincide with the opinion of the astrologers, because it is confirmed by experience; but in other respects I cannot help despising their puerile vanities.' Among the astronomers to whom he wrote was his friend Crabtree.[1]

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[1] Both these ardent students of astronomy died young. Horrox (or Horrocks, as his name is now more commonly spelt) was but twenty years old when he calculated the transit, so that his feat may not inaptly be compared to that of Adams in calculating the place of the unknown planet Neptune within a few months of taking his degree. Each instance of an early mastery of difficult problems was fated to

In what follows I quote the account given by Horrocks himself of the observations made upon this occasion, using the translation given by the Rev. Mr. Whatton.[1]

'Following the example of Gassendi,' says Horrocks, 'I have drawn up an account of this extraordinary sight, trusting that it will not prove less pleasing to astronomers to contemplate Venus than Mercury, though she be wrapt in the close embraces of the sun :

> Vinclisque nova ratione paratis
> Admisisse Deos.

Hail! then, ye eyes that penetrate the inmost recesses of the heavens, and, gazing upon the bosom of the sun with your sight-assisting tube, have dared to point out the spots on that eternal luminary! And thou, too, illustrious Gassendi above all others, hail! thou who, first and only, didst depict Hermes' changeful orb in hidden congress with the sun. Well hast thou restored the fallen credit of our ancestors, and triumphed o'er the inconstant wanderer. Behold thyself, thrice celebrated man! associated with me, if I may venture so to speak, in a like good fortune. Contemplate, I repeat, this most extraordinary phenomenon, never in

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meet with neglect; but Horrocks died before justice had been done him. Adams was quickly able to prove that his work was sound, notwithstanding the coolness with which it had been received by official astronomers. Horrocks died in 1641, in his twenty-second year. Crabtree is supposed to have been killed at the battle of Naseby Field.

[1] The memoir accompanying Mr. Whatton's translation will be found full of interest. The complete work is published by Macintosh, 24 Paternoster Row.

our time to be seen again! the planet Venus, drawn from her seclusion, modestly delineating on the sun, without disguise, her real magnitude, whilst her disc, at other times so lovely, is here obscured in melancholy gloom; in short, constrained to reveal to us those important truths, which Mercury on a former occasion confided to thee.

'How admirably are the destinies appointed! How wisely have the decrees of Providence ordered the several purposes of their creation! Thou, a profound divine, hast honoured the patron of wisdom and learning; whilst I, whose youthful days are scarce complete, have chosen for my theme the queen of love, veiled by the shade of Phœbus' light.

'Whilst I was meditating in what manner I should commence my observation of the planet Venus, so as effectually to realise my expectations, the recent and admirable invention of the telescope afforded me the greatest delight, on account of its singular excellence and superior accuracy above all other instruments. For although the method which Kepler recommends in his treatise on Optics, of observing the diameter and eclipse of the sun through a plain aperture without the aid of glasses, is very ingenious, and in his opinion, on account of its freedom from refraction, preferable to the telescope; yet I was unable to make use of it, even if I had wished to do so, inasmuch as it does not show the sun's image exactly, nor with sufficient distinctness, unless the distance from the aperture be very great, which the smallness of my apartment would not allow. Moreover, I was afraid to risk the chance of losing the observation: a misfortune which happened to Schickard and Möstling, the astronomer to the Prince of Hesse, as Gassendi tells us in his "Mercury:" for they, expecting to find the diameter of Mercury greater than it was reasonable to anticipate, made use of so large an aperture that it was impossible to distinguish the planet at all, as Schickard himself has clearly proved; and even though Venus gave promise of a larger diameter, and thereby in some measure lessened this apprehension, and I was able to adapt the aperture to my own convenience, yet in an observation that could never be repeated I preferred encountering groundless fears to the certainty of disappointment. Besides, I possessed a telescope of my own, of such power as to show even the smallest spots upon the sun, and to enable me to make the most accurate division of his disc; one which, in all my observations, I have found to represent objects with the greatest truth.

'This kind of instrument, therefore, I consider ought always to be preferred in such experiments.

'Having attentively examined Venus with my instrument, I described on a sheet of paper a circle whose diameter was nearly equal to six inches, the narrowness of the apartment not permitting me conveniently to use a larger size. This, however, admitted of a sufficiently accurate division; nor could the arc of a quadrant be apportioned more exactly, even with a radius of fifty feet, which is as great a one as any astronomer has divided; and it is in my opinion far more convenient than a larger, for although it represents the sun's image less, yet it depicts it more clearly and steadily. I divided the circumference of this circle into 360° in the usual manner, and its diameter into thirty equal parts, which gives about as many minutes as are equivalent to the sun's apparent diameter; each of these thirty parts was again divided into four equal portions, making in all 120; and these, if necessary, may be more minutely subdivided; the rest I left to ocular computation, which, in such small sections, is quite as certain as any mechanical division. Suppose, then, each of these thirty parts to be divided into 60″, according to the practice of astronomers. When the time of the observation approached I retired to my apartment, and having closed the windows against the light, I directed my telescope, previously adjusted to a focus, through the aperture towards the sun and received his rays at right angles upon the paper already mentioned. The sun's image exactly filled the circle, and I watched carefully and unceasingly for any dark body that might enter upon the disc of light.

'Although the corrected computation of Venus's motions which I had before prepared, and on the accuracy of which I implicitly relied, forbade me to expect anything before three o'clock in the afternoon of the 24th; yet since, according to the calculations of most astronomers, the conjunction should take place sooner—by some even on the 23rd—I was unwilling to depend entirely on my own opinion, which was not sufficiently confirmed, lest by too much self-confidence I might endanger the observation. Anxiously intent, therefore, on the undertaking through the greater part of the 23rd, and the whole of the 24th, I omitted no available opportunity of observing her ingress. I watched carefully on the 24th from sunrise to nine o'clock, and from a little before ten until noon, and at one in the afternoon,—being called away in the intervals by business of the highest importance, which for these ornamental pursuits I could not with propriety neglect.[1] But during all this time I saw nothing in the sun except a small and common spot, consisting as it were of three points at a distance from the centre towards the left, which I noticed on the preceding and following days. This evidently had nothing to do with Venus. About fifteen minutes past three in the afternoon, when I was again at liberty to continue my labours, the clouds, as if by Divine interposition, were entirely dispersed, and I was once more invited to the

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[1] Presumably, as Mr. Whatton points out, 'the business of the highest importance' here referred to was the duty of conducting divine service, as November 24, Old Style, was a Sunday. Mr. Whatton quotes the following passage from one of Thomas Hearne's pocket-books, dated February 8, 1723: 'Mr. Horrox, a young man, minister of Hoole, a very poor pittance, within four miles of Preston, in Lancashire, was a prodigy for his skill in astronomy, and had he lived, in all probability he would have proved the greatest man in the whole world in his profession. He had a very strange unaccountable genius, and he is mentioned with great honour by Hevelius upon account of his discovery of Venus in the sun, upon a Sunday; but being called away to his devotions and duty at church, he could not make such observations as otherwise he would have done.'

grateful task of repeating my observations. I then beheld a most agreeable spectacle, the object of my sanguine wishes, a spot of unusual magnitude and of a perfectly circular shape, which had already fully entered upon the sun's disc on the left, so that the limbs of the sun and Venus precisely coincided, forming an angle of contact. Not doubting that this was really the shadow of the planet, I immediately applied myself sedulously to observe it.

'In the first place, with respect to the inclination, the line of the diameter of the circle being perpendicular to the horizon, although its plane was somewhat inclined on account of the sun's altitude, I found that the shadow of Venus at the aforesaid hour—namely, fifteen minutes past three—had entered the sun's disc about 62° 30′, certainly between 60° and 63°, from the top towards the right. This was the appearance in the dark apartment; therefore out of doors beneath the open sky, according to the laws of optics, the contrary would be the case, and Venus would be below the centre of the sun, distant 62° 30′ from the lower limb,—or the nadir, as the Arabians term it. The inclination remained to all appearance the same until sunset, when the observation was concluded.[1]

'In the second place, the distance between the centres of Venus and the sun, I found by three observations, to be as follows:—

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[1] Horrocks observed Venus close by the place marked *i*, in Plate I. She had barely completed ingress when he first saw her, and when his observations closed she had advanced nearly two diameters of herself

The Hour	Distance of the Centres
At 3.15 by the clock	14′ 24″
,, 3.35 ,,	13′ 30″
,, 3.45 ,,	13′ 0″
,, 3.50 the apparent sunset.	

The true setting being 3.45, and the apparent about five minutes later, the difference being caused by refraction. The clock, therefore, was sufficiently correct.

'In the third place, I found, after careful and repeated observation, that the diameter of Venus, as her shadow was depicted on the paper, was larger, indeed, than the thirtieth part of the solar diameter, though not more so than the sixth, or at the utmost

along the line of transit from the place just noted. His picture is too elaborate to be given in full, but the accompanying drawing (fig. 2) serves sufficiently to show what he observed—v being the position of Venus when first observed, v′ the position she had reached when the sun was about to set.

Fig. 2.—The Transit of 1639, as observed by Horrocks.

the fifth of such a part. Therefore, let the diameter of the sun be to the diameter of Venus as 30′ to 1′ 12″. Certainly her diameter never equalled 1′ 30″, scarcely perhaps 1′ 20″, and this was evident as well when the planet was near the sun's limbs as when far distant from it.

'This observation was made in an obscure village, where I have long been in the habit of observing, about fifteen miles to the north of Liverpool, the latitude of which I believe to be 52° 20′, although by the common maps it is stated to be 54° 12′; therefore the latitude of the village will be 53° 35′, and the longitude of both 22° 30′ from the Fortunate Islands, now called the Canaries. This is 14° 15′ to the west of Uraniburg, in Denmark, the longitude of which is stated by Brahé, a native of the place, to be 36° 45′ from these islands.

'This is all I could observe respecting this celebrated conjunction during the short time the sun remained in the horizon: for although Venus continued on his disc for several hours, she was not visible to me for longer than half an hour, on account of his so quickly setting. Nevertheless, all the observations which could possibly be made in so short a time I was enabled by Divine Providence to complete so effectually that I could scarcely have wished for a more extended period. The inclination was the only point upon which I failed to attain the utmost precision; for, owing to the rapid motion of the sun, it was difficult to observe with certainty to a single degree; and I frankly confess that I neither did nor could ascertain it. But all the rest is sufficiently accurate, and as exact as I could desire.'

Horrocks was not the only observer of the transit of 1639. 'I had written,' he says, 'to my most esteemed friend William Crabtree, a person who has few superiors in mathematical learning, inviting him to be present at this Uranian banquet, if the weather permitted; and my letter, which arrived in good time, found him ready to oblige me. He therefore carefully prepared for the observation, in a manner similar to that which has been before mentioned. But the sky was very unfavourable, being obscured during the greater part of the day with thick clouds; and as he was unable to obtain a view of the sun, he despaired of making an observation, and resolved to take no further trouble in the matter. But a little before sunset—namely, about thirty-five minutes past three—the sun bursting forth from behind the clouds, he at once began to observe, and was gratified by beholding the pleasing spectacle of Venus upon the sun's disc. Rapt in contemplation, he stood for some time motionless, scarcely trusting his own senses, through excess of joy; for we astronomers have, as it were, a womanish disposition, and are overjoyed with trifles, and such small matters as scarcely make an impression upon others; a susceptibility which those who will may deride with impunity, even in my own presence; and if it gratify them, I too will join in the merriment. One thing I request: let no severe Cato be seriously offended with our follies; for, to speak poetically, what young man on earth would not, like ourselves, fondly admire Venus in conjunction with the sun, *pulchritudinem divitiis conjunctam?*

'But to return, he from his ecstasy and I from my digression. In a little while the clouds again obscured the face of the sun, so that he could observe nothing more than that Venus was certainly on the disc at the time. What he actually saw in so short a space was as follows: In the apartment Venus occupied the right side of the sun, being higher than its centre, and therefore in the heavens lower, and on the left. She was distant at the aforesaid hour—namely, thirty-five minutes past three—a sufficiently appreciable space from the sun's left limb, but Crabtree's opportunity was so limited that he was not able to observe very minutely either the distance itself or the inclination of the planet. As well as he could guess by his eye, and to the best of his recollection, he drew upon the paper the situation of Venus, which I found to differ little or nothing from my own observation; nor indeed did he err more than Apelles himself might have done in so rapid a sketch. He found the diameter of Venus to be seven parts, that of the sun being 200, which, according to my calculations, gives about 1′ 3″.

'This observation was made near Manchester, called by Antoninus, Mancunium, or Manucium, the latitude of which Mr. Crabtree makes 52° 24′; and the common tables 54° 15′; the longitude 23° 15′; or three minutes of time to the east of Liverpool, from which it is distant twenty-four miles.

'I wrote also of the expected transit to my younger brother, who then resided at Liverpool, hoping that he would exert himself on the occasion. This indeed he did, but it was in vain; for on the 24th the sky was overcast, and he was unable to see anything, although he watched very carefully. He examined the sun again on the following day, which was somewhat clearer, but with no better success, Venus having already completed her transit.

'I hope to be excused for not informing other of my friends of the expected phenomenon; but most of them care little for trifles of this kind, preferring rather their hawks and hounds, to say no worse; and although England is not without votaries of astronomy, with some of whom I am acquainted, I was unable to convey to them the agreeable tidings, having myself had so little notice. If others, without being warned by me, have witnessed the transit, I shall not envy their good fortune but rather rejoice, and congratulate them on their diligence. Nor will I withhold my praise from anyone who may hereafter confirm my observations by their own, or correct them by anything more exact.

'Venus was visible in the sun throughout nearly the whole of Italy, France, and Spain; but in none of those countries during the entire continuance of the transit.

'But America!

O fortunatos nimium, bona si sua norint.

Venus! what riches dost thou squander on unworthy regions which attempt to repay such favours with gold, the paltry product of their mines. Let these barbarians keep their precious metals to themselves, the incentives to evil which we are content to do without. These rude people would indeed ask from us too much should they deprive us of all those celestial riches, the use of which they are not able to comprehend. But let us cease this complaint, O Venus! and attend to thee ere thou dost depart.'

On which Horrocks bursts into strains of poetry, imploring Venus not to seek those barbarous regions for which, even as his eyes were gazing upon her, she was hastening. 'But ah!' he sighs, 'thou fliest,

And torn from civil life,
The savage grasp of wild untutored man
Holds thee imprisoned in its rude embrace.
Thou fliest, and we shall never see thee more;
While heaven, unpitying, scarcely would permit
The rich enjoyment of thy parting smile.
Oh! then farewell, thou beauteous queen! thy sway
May soften natures yet untamed, whose breasts,
Bereft of native fury, then shall learn
The milder virtues. We, with anxious mind,
Follow thy latest footsteps here, and far
As thought can carry us; my labours now
Bedeck the monument for future times
Which thou at parting left us. Thy return
Posterity shall witness; years must roll
Away, but then at length the splendid sight
Again shall greet our distant children's eyes.'

CHAPTER II.

THE TRANSIT OF 1761.

From the way in which Horrocks showed how the apparent place of Venus on the sun's face must be affected by the observer's position, it is tolerably clear that he would have been led to perceive how observations made from different places could be used to determine the sun's distance, had time permitted him to correspond with other astronomers. For at the beginning of Chapter VI. he says: 'I beheld Venus during the transit, not from the centre, but from the surface of the earth, therefore I observed her apparent and not her true situation. Her true situation, which chiefly concerns us, is only to be obtained by the correction of the parallaxes, into which subject I now proceed to inquire. The hypotheses of all astronomers make the parallax of Venus in so near an approach to the earth sufficiently apparent; but this I shall leave to be further considered in a separate treatise.' He then shows how the sun's distance enters into the determination of the true from the apparent position. At the end of the work he speaks again of the proposed treatise. 'I had intended,' he says, 'to offer a more extended treatise on the sun's parallax; but as the subject appears foreign to our present purpose, and cannot be dismissed with a few incomplete arguments, I prefer discussing it in a separate treatise—"*De syderum dimensione*"—which I have in hand. In this work I examine the opinions and views of others; I fully explain the diagram of Hipparchus, by which the sun's parallax is usually demonstrated, and I subjoin sundry new speculations. I also show that the hypotheses of no astronomer (Ptolemy not excepted—nor even Lansberg, who boasts so loudly of his knowledge of this subject) answer to that diagram, but that Kepler alone properly understood it. I show, in fact, that the hypotheses of all astronomers make the sun's parallax either absolutely nothing or so small that it is quite imperceptible, whereas they themselves, not understanding what they are about, come to an entirely opposite conclusion, a paradox of which Lansberg affords an apt illustration. Lastly, I show the insufficiency and uselessness of the common mode of demonstration from eclipses. I give many other certain and easy methods of proving the distance and magnitude of the sun, and I do the same with regard to the moon and the rest of the planets, adducing several new observations.'

There cannot be a doubt, I think, that had Horrocks lived to complete this treatise, the methods subsequently devised by Halley and Delisle would have been found included among the 'certain and easy methods of proving the sun's distance and magnitude.' They are so obvious, when once the connection between transits and the solar parallax has been noticed, that they could not possibly have escaped the keen insight of the young astronomer, especially as he had actually observed Venus in transit.

Passing, however, from what might have happened, let us consider how, during the interval between Horrocks's transit and the next, the idea of utilising transits for the determination of the sun's distance presented itself to astronomers.

Priority in this matter has been claimed for James Gregory; but, as Sir Edmund Beckett points out in the last edition of his 'Astronomy without Mathematics,' on insufficient grounds. In a scholium to the 87th problem of his *Optica Promota*, Gregory says that 'the problem has a very beautiful application, although perhaps laborious, in observations of Venus or Mercury when they obscure a small portion of the sun; for by means of such observations the parallax of the sun may be investigated.' But the method described in the problem, the object of which is to determine the parallaxes of two planets by observations of their conjunctions, has no practical value. I cannot understand on what grounds Prof. Grant, in his 'Physical Astronomy,' claims for Gregory the credit usually attributed to Halley. For if the mere mention of the connection between the phenomena of a transit and the solar parallax be the point insisted upon, Horrocks seems clearly to have anticipated Gregory; if the method described by Gregory be insisted upon, then, since that method never has been and never could be applied successfully, Gregory cannot be regarded as having anticipated Halley, the inventor of a practicable method. The very fact that Mercury is associated with Venus, in the sentence quoted from Gregory's work, shows how little he had grasped the idea of Halley's problem, in the solution of which transits of Mercury are useless. It is not because of the intrinsic importance of the invention that I discuss the rival claims; for I think that the approach of the transits of 1761 and 1769 would probably have forced the attention of astronomers to the very simple considerations on which the matter depends. But, as Halley had in all probability read the *Optica Promota* (Admiral Smyth thinks Halley had certainly done so[1]), the much more important question whether Halley treated Gregory with fairness is really involved. As Gregory died in 1675, only four years before Halley mentioned the utility of observations of Venus in transit, it would seriously affect our estimate of Halley's character if we adopted Prof. Grant's conclusion. I think, however, there can be very little question, when Gregory's remarks have been carefully studied, that Halley must be acquitted of all unfairness.

On November 7, 1677, Halley, stationed at St. Helena, witnessed a transit of Mercury. He noticed that the duration of the transit could be observed very exactly, and was thus led to believe that the apparent

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[1] Nevertheless, this may be doubted, as Halley was but twenty-one years old when the idea of utilising transits first occurred to him; and it was only two years later that he announced the idea.

position of the path of transit of Mercury or Venus could be very accurately determined. In 1679, in the *Catalogus Stellarum Australium*, we find his first public mention of the idea. Later, he gave it closer attention, and at last, in 1716 (three years before he became Astronomer Royal), he contributed to the Proceedings of the Royal Society the following paper[1] (I quote Ferguson's translation):—

'There are many things exceedingly paradoxical, and that seem quite incredible to the illiterate, which yet, by means of mathematical principles, may be easily solved. Scarce any problem will appear more hard or difficult than that of determining the distance of the sun from the earth, very near the truth; but even this, when we are made acquainted with some exact observations, taken at places fixed upon and chosen beforehand, will, without much labour, be effected. And this is what I am now desirous to lay before this illustrious Society (which I foretell will continue for ages), that I may explain beforehand to young astronomers, who may perhaps live to observe these things, a method by which the immense distance

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[1] 'It must be admitted,' says Grant of this essay, 'that the ability with which Halley expounded the peculiar advantages attending the determination of the solar parallax by observations of the transits of Venus, the earnestness with which he recommended the practical application of the method, and the weight of his authority on questions relating to astronomical science, were mainly instrumental in inducing the different Governments of Europe to adopt those liberal proceedings for observing the transits of 1761 and 1769 which have led to a more accurate knowledge of the dimensions of the solar system than could otherwise be hoped for.

of the sun may be truly obtained to within a five-hundredth part of what it really is.

'It is well known that the distance of the sun from the earth is by different astronomers supposed different; according to what was judged most probable from the best conjecture that each could form. Ptolemy and his followers, as also Copernicus and Tycho Brahé, thought it to be 1,200 semidiameters of the earth; Kepler, 3,500, nearly; Ricciolus doubles the distance mentioned by Kepler, and Hevelius only increases it by one-half. But the planets Venus and Mercury, having, by the assistance of the telescope, been seen in the disc of the sun, deprived of their borrowed brightness, it is at length found that the apparent diameters of the planets are much less than they were formerly supposed; and that the semidiameter of Venus, seen from the sun, subtends no more than a fourth part of a minute, or fifteen seconds, while the semidiameter of Mercury, at its mean distance from the sun, is seen under an angle only of ten seconds; that the semidiameter of Saturn, seen from the sun, appears under the same angle; and that the semidiameter of Jupiter, the largest of all the planets, subtends an angle of no more than a third part of a minute in the sun. Whence, trying the proportions, some modern astronomers have thought that the semidiameter of the earth, seen from the sun, would subtend a mean angle between that larger one subtended by Jupiter and that smaller one subtended by Saturn and Mercury; and equal to that subtended by Venus

—namely, fifteen seconds—and have thence concluded that the sun is distant from the earth almost 1,400 of the earth's semidiameters. But the same authors have, on another account, somewhat increased this distance: for inasmuch as the moon's diameter is a little more than a fourth part of the diameter of the earth, if the sun's parallax should be supposed fifteen seconds, it would follow that the body of the moon is larger than that of Mercury; that is, that a secondary planet would be greater than a primary, which would seem inconsistent with the uniformity of the mundane system. And, on the contrary, the same regularity and uniformity seems scarcely to admit that Venus, an inferior planet, that has no satellite, should be greater than our earth, which stands higher in the system, and has such a splendid attendant. Therefore, to observe a mean, let us suppose the semidiameter of the earth seen from the sun, or, which is the same thing, the sun's horizontal parallax, to be twelve seconds and a half—according to which the moon will be less than Mercury, and the earth larger than Venus—and the sun's distance from the earth will come out nearly 16,500 of the earth's semidiameters. This distance I assent to at present as the true one, till it shall become certain what it is by the experiment which I propose. Nor am I induced to alter my opinion by the authority of those (however weighty it may be) who are for placing the sun at an immense distance beyond the bounds here assigned, relying on observations made upon the vibrations of a pendulum, in order to determine those exceeding small angles; but which, as it seems, are not sufficient to be depended upon; at least, by this method of investigating the parallax, it will come out sometimes nothing, or even negative—that is, the distance would either become infinite, or greater than infinite, which is absurd. And indeed, to confess the truth, it is hardly possible for a man to distinguish, with any degree of certainty, seconds, or even ten seconds, with instruments, let them be ever so skilfully made. Therefore it is not at all to be wondered at that the excessive nicety of this matter has eluded the many and ingenious endeavours of such skilful operators.

'About forty years ago, when I was in the island of St. Helena, observing the stars about the south pole, I had an opportunity of observing, with the greatest diligence, Mercury passing over the disc of the sun; and (which succeeded better than I could have hoped for) I observed, with the greatest degree of accuracy, by means of a telescope twenty-four feet long, the very moment when Mercury, entering upon the sun, seemed to touch its limb within, and also the moment when going off it struck the limb of the sun's disc, forming the angle of interior contact; whence I found the interval of time, during which Mercury then appeared within the sun's disc, even without an error of one second of time. For the lucid line intercepted between the dark limb of the planet and the bright limb of the sun, although exceedingly fine, is seen by the eye, and the little dent made on the sun's limb, by Mercury's entering the disc, appears to vanish in a moment; and also that made by Mercury leaving the disc seems to begin in an instant. When I perceived this it immediately came into my mind that the sun's parallax might be accurately determined by such kinds of observations as these, provided Mercury were but nearer to the earth, and had a greater parallax from the sun; but the difference of these parallaxes is so little as always to be less than the solar parallax which we seek, and therefore Mercury, though frequently to be seen on the sun, is not to be looked upon as fit for our purpose.

'There remains, then, the transit of Venus over the sun's disc; whose parallax, being almost as great as the solar parallax, will cause very sensible differences between the times in which Venus will seem to be passing over the sun at different parts of the earth. And from these differences, if they be observed as they ought, the sun's parallax may be determined even to a small part of a second. Nor do we require any other instruments for this purpose than common telescopes and clocks, only good of their kind; and in the observers nothing more is needful than fidelity, diligence, and a moderate skill in astronomy. For there is no need that the latitude of the place should be scrupulously observed, nor that the hours themselves should be accurately determined with respect to the meridian; it is sufficient that the clocks be regulated according to the motion of the heavens, if the times be well reckoned from the total ingress of Venus into the sun's disc to the beginning of her egress from it; that is, when the dark globe of Venus first begins to touch the bright limb of the sun within; which moments I know, by my own experience, may be observed within a second of time.

'But, on account of the very strict laws by which the motions of the planets are regulated, Venus is seldom seen within the sun's disc; and during the course of 120 years it could not be seen once—namely, from the year 1639 (when this most pleasing sight happened to that excellent youth Horrocks, our countryman, and to him only since the Creation) to the year 1761, in which year, according to the theories which we have hitherto found agreeable to the celestial motions, Venus will again pass over the sun on May 26,[1] in the morning; so that at London about five o'clock in the morning we may expect to see it near the middle of the sun's disc, and not above four minutes of a degree south of the sun's centre.[2] But the dura-

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[1] June 6, according to new style.

[2] The true time of mid-transit was almost twenty-three minutes past five, and Venus, instead of being only 4′ south of the sun's centre at mid-transit, passed more than $9\frac{1}{2}'$ below that point. The difference in the latter respect was much the more important. Halley was not unaware of the possibility of error in his computation, since the error arose from his neglecting the shifting of the nodes of Venus, described farther on (p. 108); and he notes that possibly the nodes may shift.

Any exact discussion of the phenomena which the transit would have presented if Halley's computations had been correct would, of course, be idle; but it may be as well roughly to indicate the actual difference between the transit as it occurred and as Halley computed it.

In fig. 3, C is the centre of the sun's disc, i E; i e is the path of tion of this transit will be almost eight hours—namely, from two o'clock in the morning till almost ten. Hence

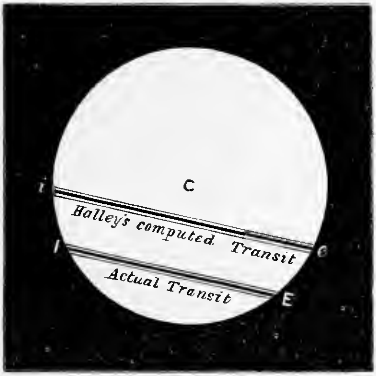

Fig. 3.—Illustrating the Transit of Venus in 1761, as it actually occurred, and as Halley computed it.

Venus, as computed by Halley; I E is the path she actually traversed. The time occupied in traversing I E was about 6¼ hours, whereas the time which would have been occupied in traversing *i e* amounts to close upon eight hours, being very little less than that occupied in a central transit. It is manifest, at once, that the chords of transit *i e* are much more nearly equal than the chords, I E, so that as far as mere length of transit chord is concerned it would be useless to set the observers far apart in a northern and southern direction. But what Halley hoped to do was this:—

Let P, fig. 4, be the North pole of the earth, travelling in the direction indicated by the arrow, P being in sunlight, as the date is June 6. The equator is represented by *e′* E *e*. Now, suppose for a moment that an observer at E sees Venus in the direction E V (Venus herself being supposed to lie far beyond the picture on the right, and above the level of the paper, to correspond to the shape given to the terminator between light and darkness on the earth). Then, at this moment an observer at A sees Venus in direction A *v*, or apparently not so far advanced (since she comes between the earth and sun, moving in the same direction as the earth around the sun, and with a greater velocity). On the other hand, the observer at A′ sees her in direction A′ *v′*, or apparently farther the ingress will not be visible in England; but as the sun will at that time be in the sixteenth degree of

advanced. Hence the effect of being carried from A to A′ is to throw Venus forward on her path. But an observer at A, when transit began,

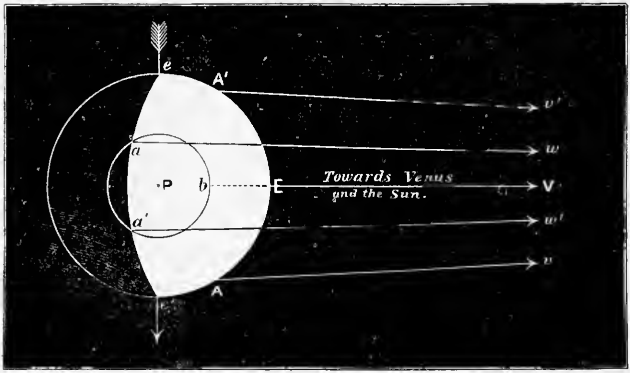

Fig. 4.—Illustrating the Conditions of the Transit of 1761, as computed by Halley.

would be carried by the earth's rotation, during the transit (lasting nearly eight hours) to the position A′; to him, therefore, the duration of the transit would be shortened by the earth's rotation. But next consider three observers in the latitude parallel *a′ b a*, near to the pole. The observer at *b* sees Venus in direction *b* V; from *a* she is seen towards *w*, or thrown forwards; from *a′* she is seen towards *w′*, or thrown backwards. The effect of being carried from *a* to *a′* is, then, to throw Venus back, or lengthen the duration of her transit. Now, if we set an observer so that at the beginning of the transit he is at *a*, he will be carried to the position *a′* at the end of the transit, if only we so select the latitude parallel *a a′* that the part in the darkened hemisphere corresponds to rather less than eight hours' rotation; in other words, take a latitude where, on June 6, or 15 days before midsummer, the night lasts less than eight hours. We find latitude 56° North suitable. This would give the beginning of the transit at sunset and the end at sunrise; and the whole of the transit between the contacts invisible. But as the sun must not be exactly on the horizon at the critical moments, we must take a place in somewhat higher latitude than 56°; and of course, the

Gemini, having almost twenty-three degrees north declination, it will be seen without setting at all,

longitude of *a*, as of A, would depend on the time at which transit began, since we must have the station which is at A at the beginning carried to the position E, at the middle. According to Halley's computation the middle of the transit would occur at about 5 in the evening, or E must be in seven hours east longitude at mid-transit. This, then, is the longitude of the equatorial station; and the longitude of the northern station is therefore to be in five hours west longitude.

Plate IV. would have to be thus altered to illustrate the circumstances of the transit as computed by Halley:—The two projections, instead of touching in Sumatra, should touch about a third of an hour farther east; since C A corresponds to the length of transit, the points A and D should be brought nearly two hours in longitude nearer together; and of course A′ and D′ should be shifted to correspond. The point I would move to a place near the new position of A′, I′ to a point near the new position of B, E near to the new position of D, and E′ near the new position of C′. Thus, H, which is the middle of the arc E I, would come close to Sumatra, and H′ would be near the Galapagos Islands. It would have been easy to find a number of stations near H in its new position; but the region *e m i*, much increased by the shifting of A B and C D, would be the best part, so far as approach to the new position of H′ was concerned. It will be seen that, under the actual conditions of the transit, the region *i m e* was quite unsuited for the purpose which

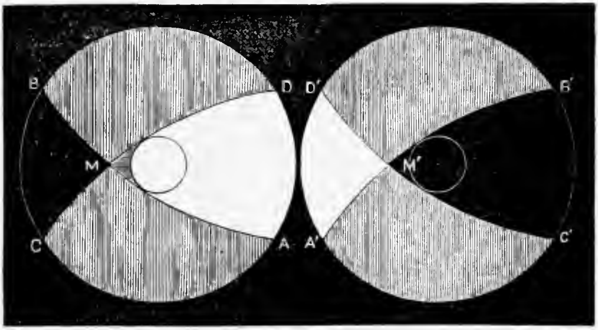

Fig. 5.—Illustrating the changes to be made in Plate IV. in order that it may correspond to the transit of 1761, as computed by Halley.

in almost all parts of the north frigid zone; and therefore the inhabitants of the north coast of Norway, beyond the city of Nidrosia, which is called Drontheim, as far as the North Cape, will be able to observe Venus entering the sun's disc; and perhaps the ingress of Venus upon the sun when rising will be seen by the Scotch, in the northern parts of the kingdom, and by the inhabitants of the Shetland Isles, commonly called Thule. But at the time when Venus will be nearest the sun's centre the sun will be vertical to the northern shores of the Bay of Bengal, or rather over the kingdom of Pegu; and therefore in the adjacent regions, as the sun, when Venus enters his disc, will be almost four hours towards the east, and as many towards the west at the time of her egress, the apparent motion of Venus on the sun will be accelerated by almost double the horizontal parallax of Venus from the sun; because Venus at that time is carried with a retrograde motion from east to west, while an eye placed upon the earth's surface is whirled the contrary way, from east to west. Supposing the sun's parallax (as we have said) to be $12\frac{1}{2}''$, the parallax of Venus will be $43''$; from which, subtracting the parallax of the sun, there will remain $30''$ at least for the horizontal parallax of Venus from the sun; and therefore the motion of Venus will be increased $45''$ at least by that parallax, while she passes over the sun's disc in those elevations of the pole

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had led Halley to indicate it for occupation; and the nearest approach to H′ was within the space D′ m' A′, near m'. Fig. 5 illustrates the conditions of transit as computed by Halley.

which are in places near the tropic, and yet more in the neighbourhood of the equator. Now, Venus at that time will move on the sun's disc very nearly at the rate of four minutes of a degree in an hour; and therefore eleven minutes of time at least are to be allowed for 45″, or three-fourths of a minute of a degree; and by this space of time the duration of this eclipse caused by Venus will, on account of the parallax, be shortened. And from this shortening of the time only we might safely enough draw a conclusion concerning the parallax which we are in search of, provided the diameter of the sun and the latitude of Venus were accurately known. But we cannot expect an exact computation in a matter of such subtility.

'We must endeavour, therefore, to obtain if possible another observation, to be taken in those places where Venus will be in the middle of the sun's disc at midnight; that is, in places under the opposite meridian to the former, or about six hours or ninety degrees west of London, and where Venus enters upon the sun a little before its setting, and goes off a little after its rising. And this will happen under the above-mentioned meridian, and where the elevation of the north pole is about fifty-six degrees; that is, in a part of Hudson's Bay near a place called Port Nelson. For, in this and the adjacent places, the parallax of Venus will increase the duration of the transit by at least six minutes of time; because while the sun from its setting and rising seems to pass under the pole, those places on the earth's disc will be carried with a motion from east to west contrary to the motion of the Ganges; that is, with a motion conspiring with the motion of Venus; and therefore Venus will seem to move more slowly on the sun, and to be longer in passing over its disc.

'If therefore it should happen that this transit should be properly observed by skilful persons at both these places, it is clear that its duration will be seventeen minutes longer as seen from Port Nelson, than as seen from the East Indies. Nor is it of much consequence (if the English shall at that time give any attention to this affair) whether the observation be made at Fort George, commonly called Madras, or at Bencoolen, on the western shore of the island of Sumatra, near the equator. But if the French should be disposed to take any pains herein, an observer may station himself conveniently enough at Pondicherry, on the west shore of the Bay of Bengal, where the altitude of the pole is about twelve degrees. As to the Dutch, their celebrated mart at Batavia will afford them a place of observation fit enough for the purpose, provided they also have but a disposition to assist in advancing, in this particular, the knowledge of the heavens. And indeed I could wish that many observations of this famed phenomenon might be taken by different persons at separate places, both that we might arrive at a greater degree of certainty by their agreement, and also lest any single observer should be deprived by the intervention of clouds of a sight which I know not whether any man living in this or the next age will ever see again; and on which depends the certain and adequate solution of a problem the most noble, and at any other time not to be attained to. I recommend it therefore again and again to those curious astronomers who (when I am dead) will have an opportunity of observing these things, that they would remember this my admonition, and diligently apply themselves with all their might in making this observation, and I earnestly wish them all imaginable success: in the first place, that they may not by the unseasonable obscurity of a cloudy sky be deprived of this most desirable sight, and then, that having ascertained with more exactness the magnitudes of the planetary orbits, it may redound to their immortal fame and glory.

'We have now shown that by this method the sun's parallax may be investigated to within its 500th part, which doubtless will appear wonderful to some. But if an accurate observation be made in each of the places above marked out, we have already demonstrated that the durations of this eclipse made by Venus will differ from each other by 17 m. of time; that is, upon a supposition that the sun's parallax is $12\frac{1}{2}''$. But if the difference shall be found by observation to be greater or less, the sun's parallax will be greater or less nearly in the same proportion. And since 17 m. of time are answerable to $12\frac{1}{2}''$ of solar parallax, for every second of parallax there will arise a difference of more than 80 s. of time; whence if we have this difference true to two seconds it will be certain what the sun's parallax is to within a 40th part of 1″; therefore his distance will be determined to within its 500th part at least, if the parallax be not found less than what we have supposed: for 40 times 12½ make 500.

'And now I think that I have explained this matter fully, and even more than I needed to have done to those who understand astronomy; and I would have them take notice that on this occasion I have had no regard to the latitude of Venus, both to avoid the inconvenience of a more intricate calculation, which would render the conclusion less evident, and also because the motion of the nodes of Venus is not yet discovered, nor can be determined but by such conjunctions of the planet with the sun as this is. For we conclude that Venus will pass four minutes below the sun's centre, only in consequence of the supposition that the plane of Venus's orbit is immovable in the sphere of the fixed stars, and that its nodes remain in the same places where they were found in the year 1639. But if Venus in the year 1761 should move over the sun in a path more to the south, it will be manifest that her nodes have moved backwards among the fixed stars; and if more to the north, that they have moved forwards; and that at the rate of 5½′ of a degree in 100 Julian years, for every minute that Venus's path shall be more or less distant than the above-said 4′ of the sun's centre. And the difference between the duration of these eclipses will be somewhat less than 17 m. of time, on account of Venus's south latitude; but greater if by the motion of the nodes forwards she should pass on the north of the sun's centre.'

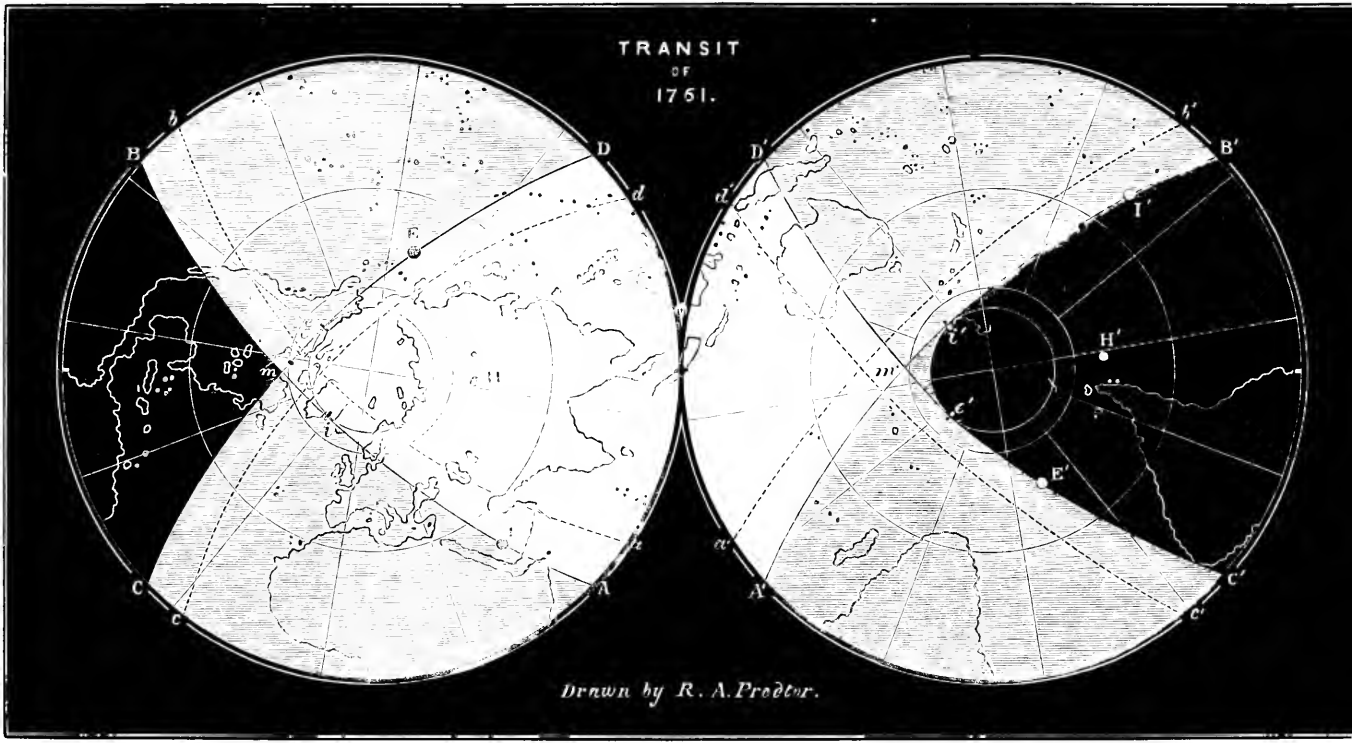
The rest of Halley's dissertation I omit, because it relates to the details of the transit as incorrectly computed by him, and therefore possesses no present interest.

As I have said it was not until three years after his essay appeared that Halley became Astronomer Royal. It does not appear that during the remaining years of his life he made any farther contribution to the subject. He died on January 14, 1742, more than nineteen years before the transit occurred.

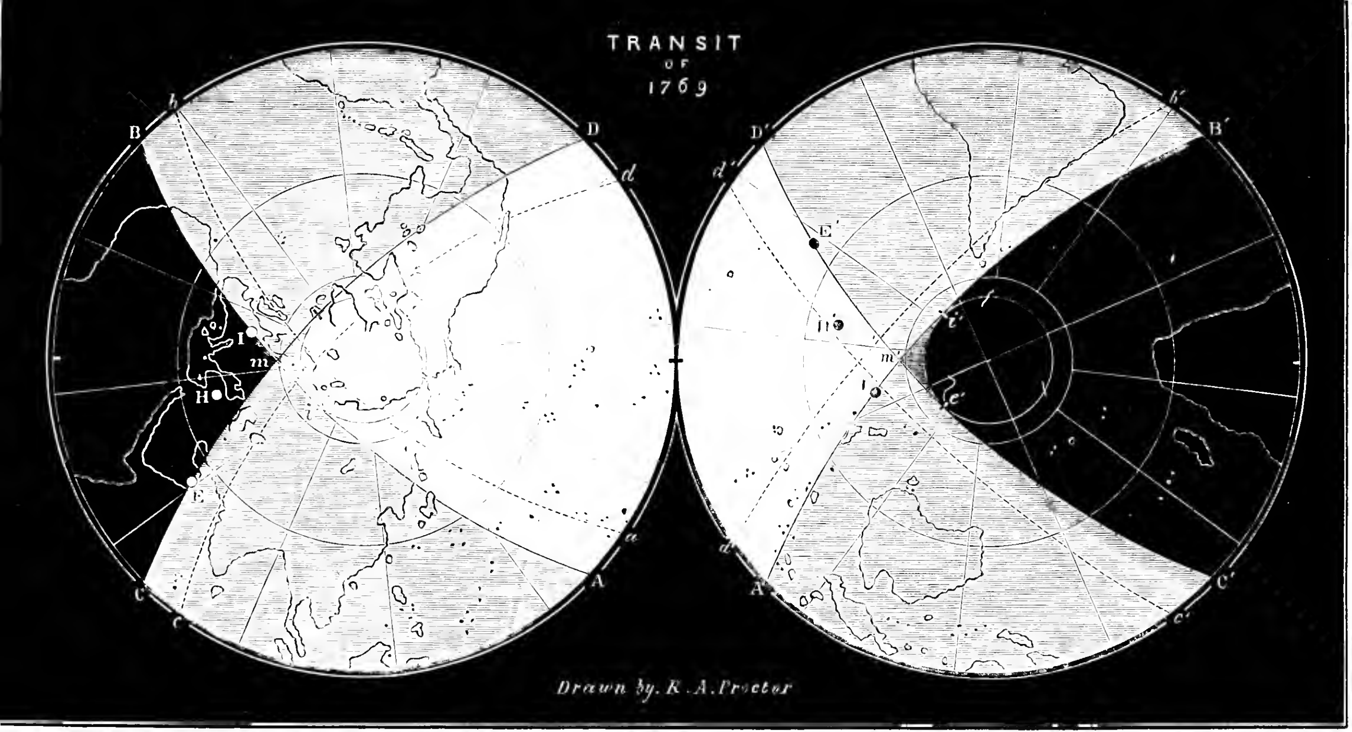
As the time for the transit drew near astronomers began to examine carefully the motions of Venus, in order to ascertain how far the conditions on which Halley's computation had been based were really fulfilled. Passing over, however, a paper by Trébuchet, pointing out inaccuracies in Halley's dissertation, it was not until August 1760, or less than a year before the transit took place, that the conditions on which successful observation depended were pointed out by Delisle. He published a chart of the earth on an equatorial projection, showing the hour at which the transit would begin or end. The chart corresponded, in fact, to Plate IV., meridional projections being substituted for the equatorial projections there used. It will be understood, however, that Delisle did not claim for his chart the degree of accuracy aimed at in Plate IV. He showed that the stations selected by Halley were not suited to the actual conditions of the transit, and that in fact the transit could not be well observed by the method of durations. He showed how, at suitably selected stations, whose longitude had been accurately determined, the single observation of a contact, whether at ingress or egress, would supply the means of determining the solar parallax. For the description of his method the reader is referred to Chapter IV.

Ferguson, in England, seems to have independently arrived at the same conclusion, not long after; at least his treatise on the subject suggests the impression that he had selected his own method of dealing with it, and had carried his analysis nearly to its completion when Delisle's paper and map reached him. He found that 'instead of passing only four minutes of a degree below the sun's centre, Venus will pass almost ten minutes of a degree below it, on which account the line of the transit will be so much shortened as will make her passage over the sun's disc about an hour and twenty minutes less than if she passed only four minutes below the sun's centre at the middle of her transit; and therefore her parallax from the sun will be so much diminished, both at the beginning and end of her transit, and at all places from which the whole of it will be seen, that the difference of its duration, as seen from them, and as supposed to be seen from the earth's centre, will not amount to eleven minutes of time. But this is not all; for although the transit will begin before the sun sets to Port Nelson, it will be quite over before he rises to that place next morning, on

PLATE IV.



A B and A′ B′ separate sunlit and darkened hemispheres at ingress ; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress ; along *c d*, *c′ d′*, sun 10° high at egress.



A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along *c d*, *c′ d′*, sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

account of its ending so much sooner than as given by the tables to which Dr. Halley was obliged to trust. So that we are quite deprived of the advantage that otherwise would have arisen from observations made at Port Nelson.'

Ferguson gave a chart of the transit on the same plan as I have used in Plates II.–IX. The chart was taken directly from Delisle, however, as Ferguson tells us, only 'I have changed,' he says, 'his meridional projections into that of the equatorial; by which I apprehend that the curved lines showing at what places the transit begins or ends with the rising or setting sun appear more natural to the eye and are more fully seen at once than in the map from which I copied; for in that map the lines are interrupted and broken in the meridian that divides the hemisphere, and the places where they should join cannot be perceived so readily by those who are not well skilled in the nature of the stereographical projections.' It shows how clear an insight Ferguson had obtained into the conditions of the transit, that, commenting on his charts, in which the line, B A of Plate IV., passes down the eastern shore of the Red Sea, while A′ B′ crosses Madagascar, he says: 'I question much whether the transit will begin at sunrise to any place in Africa that is west of the Red Sea, and am pretty certain that the sun will not be risen to the northernmost part of Madagascar when the transit begins, so that the line,' corresponding to A B, A′ B′ of Plate IV., 'seems to be a little too far west in the map at all places which are south of Asia Minor; but in Europe I think it is very well.'

The actual circumstances of the transit of 1761 in different parts of the earth can be inferred with sufficient accuracy from what is shown in Plate IV. Here the arcs A I B and A′ I′ B′ separate the dark and light hemispheres of the earth at the beginning of the transit, while the arcs C E D and C′ E′ D′ separate the dark and light hemispheres at the end of the transit. Thus the beginning of transit was visible at all places on the hemisphere formed by combining the sections A I B D and A′ I′ B′ D′, and the end of the transit was visible at all places on the hemisphere formed by combining the sections C E D A and C′ E′ D′ A′. The whole of the transit was visible over the spaces D *e i* A and D′ *m′* A′; but in the space *i m e*, though the beginning and end of the transit were seen, the progress of the transit was not wholly visible. No part of the transit was visible over the spaces B *m* C and B′ *i′ e′* C′; but in the space *i′ m′ e′*, though neither the beginning nor the end were visible, the progress of the transit was partially visible. At all points of the arcs A I B and A′ I′ B′ the ingress occurred with the sun on the horizon; but whereas the sun was rising for the arcs A *i* and A′ *i′*, he was setting for the arcs B *i* and B′ *i′*: at all points of the arcs C E D and C′ E′ D′ the egress occurred with the sun on the horizon; but whereas the sun was rising for the arcs C *e* and C′ *e′*, he was setting for the arcs D *e* and D′ *e′*. At the points *m* and *m′* the sun was on the horizon both at ingress and at egress; but whereas the progress of the whole transit, except ingress and egress, took place during the night at *m*, it took place during the day at *m′*. In all that has here been said, the passage of Venus's centre has been alone considered.

The point I′ was that where ingress occurred earliest, the point I being that where ingress occurred latest. It was around these points, then, that observers of ingress by Delisle's method were to be placed, keeping, of course, to that side of the arcs A′ B′ and A B on which the sun would be above the horizon at the time of ingress. We see that several islands were conveniently placed near I′ for showing accelerated ingress, though they were not very well known in those days. The eastern parts of Arabia and parts of India afforded convenient stations near I for observing retarded ingress.

The point E was that where egress occurred earliest, E′ being that where egress occurred latest. Kamschatka, Japan, and Manchooria afforded convenient stations for observing accelerated egress, while the Cape of Good Hope was well placed for observing retarded egress.[1]

As regarded the application of Halley's method—that is, the observation of duration where greatest and least—the transit was not a favourable one. H was the

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[1] Encke, in 1822, found the following elements for the transit of 1761. I quote them from the excellent little treatise, 'Les Passages de Vénus sur le disque Solaire,' by M. Dubois, Naval Examiner in Hydrography for France, substituting Greenwich for Paris time and longitude,

place where transit had the shortest duration, H′ being the place where, if the transit had been visible, its duration would have been least. Stations near H could of course be occupied, as here the summer of Siberia was in progress. But we see that there was no station at all near to H′ whence the whole transit could be seen. The point *m′* was geometrically the most advantageous, but there the sun was upon the horizon. The south-western extremity of Australia or the island of St. Paul were the only regions available, and they were almost as far from H′ as from H. In point of fact, Halley's method failed totally on this occasion. It commonly fails at the earlier transit of a pair separated by eight years, as will be shown in the next chapter; but it is worthy of notice that the circumstances of the transit of 1761 in this respect were very much like those of the coming transit of 1882. Although the transit of 1882 will be the

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and correcting a misprint, by which in one place north and south latitudes are interchanged :—

	h.	m.	s.	
Ingress of Venus's centre	14	12	48·5	Greenwich apparent solar time.
Middle of the transit	17	23	0·0	
Egress of the centre	20	29	13·5	
Duration of the transit	6	16	25·0	
Least distance of centres. . . .	0	9	34·2	

	Latitude		Longitude	
	°	′	°	′
Pole of accelerated ingress	20	56 S	132	28 W
,, ,, retarded ,, ...	20	56 N	47	32 E
,, ,, accelerated egress	46	47 N	167	59 E
,, ,, retarded ,, ...	46	47 S	12	1 W
,, ,, shortened durations	52	31 N	92	42 E
,, ,, lengthened ,, ...	52	31 S	87	18 W

second transit of a pair, its geometrical superiority is counterbalanced by the inaccessibility of the antarctic as compared with the arctic regions.

I do not know that any useful purpose could be served by inserting here an account of the various observations of the transit of 1761 made by the persons, 176 in number, who took a more or less important share in the work at no less than 117 stations. Presently the peculiar phenomena which rendered the observation of internal contact uncertain will be described; but the mere records of time observations have no special interest. A few examples may suffice to show this.

We see from Plate IV. that the beginning of the transit was invisible in the western parts of Europe, but the latter half was visible there, though not under specially advantageous circumstances. We have the following particulars respecting the observations in London at Greenwich: 'Early in the morning, when every astronomer was preparing for observing the transit, it unluckily happened that, both at London and the Royal Observatory at Greenwich, the sky was so overcast with clouds as to render it doubtful whether any part of the transit would be seen, and it was 38 m. 21 s. past 7 o'clock, apparent time, at Greenwich when the Rev. Mr. Bliss, our Astronomer Royal, first saw Venus on the sun. . . From that time to the beginning of egress the Doctor made several observations, both of the difference of right ascension and declination of the centres of the sun and Venus, and at last found the beginning of egress, or instant of the internal contact of Venus with the sun's limb, to be at 8 hours 19 minutes 0 seconds apparent time. . . . By the means of three good observations the diameter of Venus on the sun was 58 seconds of a degree.' 'Mr. Short made his observations at Savile House, in London, 30 seconds in time west of Greenwich, in presence of His Royal Highness the Duke of York, accompanied by Their Royal Highnesses Prince William, Prince Henry, and Prince Frederick.' So the account runs. We are not told whether the Duke of York actually honoured Venus by directing His Royal gaze upon her during her transit, or whether Their Other Royal Highnesses made any observations; but as Venus was under observation for about 3½ hours, we may suppose that these exalted persons did not lose the opportunity of witnessing a phenomenon so seldom seen. Venus, all unconscious of the honour, moved onwards to egress, contact occurring at 8 h. 18 m. 15½ s. apparent Greenwich time, or 8½ s. sooner than at Greenwich. At Stockholm the whole transit was observed by Wargentin, the whole duration (between the internal contacts) being 5 h. 50 m. 45 s., corresponding to a little over six hours for the passage of the centres. At Stockholm, as we see from Plate IV., the transit was shortened as compared with the mean duration.

Chappe d'Auteroche was stationed at Tobolsk, in Siberia—an important station for the Halleyan method (see Plate IV.), if any stations had been available for observing lengthened durations. The transit, as observed by him, lasted 5 h. 48 m. 32½ s., or nearly

m. less than at Stockholm. Chappe had some ouble in reaching Tobolsk in time for his observations. Ie started at the end of November 1760, and reached t. Petersburg readily enough; but the journey ence to Tobolsk was not completed without inconenience and even serious dangers. He reached obolsk on April 10, 1761, the voyage having lasted ve months.

England sent out an expedition intended for Benoolen, in Sumatra, apparently because that station had een mentioned in Halley's dissertation; for Sumatra, lmost midway between II and *m′* (Plate IV.), offered o advantages for the observation of durations, and was ltogether too far removed both from I and E to be of e least service as a Delislean station. Fortunately the hip was attacked by a Spanish war-ship on the road, nd had to put in at the Cape, where very useful obserations of the retarded egress were made. Another nglish expedition was sent to St. Helena, a station here retarded egress was observable, but by no eans advantageously. At Madras, Mr. Hirst, and t Calcutta, Mr. Magee (whom M. Dubois converts to Magec) observed the duration of transit, obtaing respectively the periods 5 h. 51 m. 43 s., and 5 h. 0 m. 36 s., values which differ much more from each ther than parallax will account for. As Ferguson well emarks of the whole series of observations: 'Whoever ompares the times of the internal contacts, as given y different observers, will find such difference among hem, even those which were taken from the same spot, as will show that the instant of either contact could not be so accurately perceived by the observers as Dr. Halley thought it could, which probably arises from the difference of people's eyes and the different magnifying powers of those telescopes through which the contacts were seen. If all the observers had made use of equal magnifying powers there can be no doubt that the times would have more nearly coincided, since it is plain that, supposing all their eyes to be equally quick and good, they whose telescopes magnified most could perceive the point of internal contact soonest and of the total exit latest.'

Le Gentil, who had been appointed to observe at Pondicherry, was very unfortunate. The following account is taken from M. Dubois' admirable work already referred to: 'On account of the distance of the station where he was to observe the transit, Le Gentil set out from France on March 26, 1760. The observation he hoped to make at Pondicherry was curious and interesting, says J. D. Cassini; in fact, he would have seen the whole transit, and the middle would have occurred when the sun was nearly on the meridian at about ten degrees from the zenith. Le Gentil arrived at the Isle of France on July 10, 1760, that is to say, nearly a year before the expected transit; but the war which arose at that epoch between France and England rendered it no longer possible for him to go to Pondicherry. He resolved to betake himself to Rodriguez, awaiting meanwhile the progress of events. He was just setting off for this new station, where also De Pingré was to observe, when he learned that a French frigate was about to leave the Isle of France for the coast of Coromandel. Le Gentil resolved to avail himself of this opportunity to go to the place selected by the Academy of Sciences; but he was not able to leave the Isle of France on board this frigate till about the middle of March 1761. It was already very late. The frigate carrying the French astronomer experienced at first long-continued calms, which were enough to cause Le Gentil to despair, and which did not permit him to reach the coast of Malabar before May 24. To increase his ill-fortune, the commander of the frigate learned that the English were masters of Mahe and Pondicherry. The frigate had no other resource but to take flight without delay. This she did; and, to the utter despair of Le Gentil, she retook her way towards the Isle of France. The 6th of June arrived! The frigate was in 87° East longitude (from Paris), and 5° 45′ South latitude. The sky was clear, the sun splendid! The unfortunate Le Gentil, unwilling to be altogether idle, observed the transit on board the ship, taking all possible care. He noted the times of ingress and egress; but with what degree of approximation were those times obtained, even admitting that those he noted coincided exactly with the instant of the contacts? The voyage of the French Academician ended thus in failure. Le Gentil then experienced one of those mishaps which assume to the man of science all the proportions of a real misfortune—to have traversed so large a portion of the globe, to have endured all the weariness, all the privations, all the perils of a long sea-voyage, and to effect nothing! This was enough to have disgusted anyone with scientific observation, or at least with Halley's method. We shall presently see, however, when dealing with the transit of 1769, that Le Gentil, so far as that method was concerned, had not yet seen the last of his troubles.'

De Pingré reached Rodriguez in May 1761; and although he had to observe in the open air, and could scarcely find a place where to keep his clock out of the wind, his observations were among the best of those effected during the transit of 1761.

The results of the observations were far from satisfactory, the values of the solar parallax deduced by mathematicians ranging between 8″·5 and 10″·5, corresponding to a distance of the sun ranging from 96,162,840 miles to 77,846,110 miles. From a comparison of a great number of observations made by Short the parallax 8″·5 was deduced for the day of the transit, corresponding to 8″·65 for the earth's mean parallax, or a distance of 94,498,420 miles. In 1822, Encke, then sub-director at Seeberg, deduced from the observations made in 1761 a parallax lying between the extreme limits 8″·429813 and 8″·551237, corresponding to the distances 97,000,000 miles and 95,600,000 miles.

These discrepancies were no doubt due to two chief causes. In the first place, the observations were mostly Delislean, and in the last century means did not exist for the determination of the longitude with the degree of accuracy which was required. Secondly, it was found that the phenomena attending the ingress and egress of Venus are not so simple as Halley had supposed, when he stated that the time of internal contacts can be determined within a single second of time. Halley had reckoned on the appearances presented during a transit such as he had observed at St. Helena, when the sun was high above the horizon, and the small disc of Mercury was little disturbed by atmospheric effects. But at most of the stations for effectively observing the transit of Venus in 1761, and at all those best suited for applying Delisle's method, the sun was not far from the horizon, and the outline of Venus was seriously affected by atmospheric undulations. Moreover, an optical phenomenon which had not attracted Halley's attention was presented during the transit of 1761, and caused the observations to be much less reliable than they would otherwise have been. The disc of Venus was found to assume near the time of internal contact a distorted form. In some cases she seemed to be attached to the edge of the sun by a dark ligament of greater or less breadth, as shown at 1, 2, and 3, fig. 6; in other cases she

Fig. 6.—Illustrating the 'Black Drop.'

appeared shaped like a pear, while in others she was altogether distorted by the combined effect of atmospheric disturbances and the optical distortion (whatever its real nature may be) which causes the black drop and pear-shape figures.

This was the first occasion on which the peculiar appearances in question were noted; but as the difficulty thus introduced affected the discussion of the observations made during both the transits of the last century, this will be a convenient place for describing what was seen in 1769 as well as in 1761. Professor Grant has collected together in his fine 'History of Physical Astronomy' several of the most interesting observations of this kind, and from his work I quote the following cases:—

Mr. Hirst, who observed the transit of 1761 at Madras, stated that 'at the total immersion the planet, instead of appearing truly circular, resembled more the form of a bergamot pear, or, as Governor Pigott then expressed it, *looked like a ninepin*; yet the preceding limb of Venus was extremely well defined.' With respect to the end of the transit, he remarked 'that the planet was as black as ink, and the body truly circular, just before the beginning of egress, yet it was no sooner in contact with the sun's preceding limb, than it assumed the same figure as before at the sun's subsequent (following) limb; the subsequent limb of Venus keeping well-defined and truly circular.'

A similar appearance was observed by Salunde at Paris, by Bergman at Upsal, and also by several other individuals.

Dr. Maskelyne, who observed the transit of 1769 at Greenwich, gives the following description of a phenomenon of a similar nature witnessed by him at the egress of the planet:—

'The regularity of Venus's circular figure was disturbed towards the place where the internal contact should happen by the addition of a protuberance dark like Venus and projecting outwards, which occupied a space upon the sun's circumference which bore a considerable proportion to the diameter of Venus. Fifty-two seconds before the thread of light was formed, Venus's regular circumference (supposed to be continued as it would have been without the protuberance) seemed to be in contact with the sun's circumference, supposed also completed. Accordingly, from this time Venus's regular circumference (supposed defined in the manner just described) appeared wholly within the sun's circumference, and it seemed, therefore, wonderful that the thread of light should be so long before it appeared, the protuberance appearing in its stead. At length when a considerable part of the sun's circumference (equal to one-third or one-fourth of the diameter of Venus) remained still obscured by the protuberance, a fine stream of light flowed gently round it from each side, and completed the same in the space of three seconds of time. But the protuberance, though diminished, was not taken away till about twenty seconds more; when, after being gradually reduced, it disappeared, and Venus's circular figure was restored.

Dr. Bevis states in the account of his observations that 'the planet seemed quite entered upon her disc, her upper limb being tangential to that of the sun; but instead of a thread of light, which he expected immediately to appear between them, he perceived Venus to be still conjoined to the sun's limb by a slender tail, nothing near so dark as her disc, and shaped like the neck of a Florence flask. The said tail vanished at once; and for a few seconds after, the limb of Venus, to which it had been joined, appeared more prominent than her lower part, somewhat like the lesser end of an egg, but soon resumed its rotundity.'

The Rev. Mr. Hirst thus describes the appearance presented during the transit: 'The same phenomena of a protuberance which I observed at Madras in 1761, at both internal contacts, I observed again at this last transit. At both times the protuberance of the upper edge of Venus diminished nearly to a point before the thread of light between the concave edge of the sun and the concave edge of the planet was perfected, when the protuberance broke off from the upper edge of the sun, but Venus did not assume its circular form till it had descended into the solar disc some distance.'

Mr. Dunn, who observed the transit at Greenwich, remarks that 'he saw the planet held as it were to the sun's limb by a ligament formed of many black cones whose bases stood on the limb of Venus, their vertices pointing to the limb of the sun.'

'Mr. Pigott states that Venus, before she separated from the sun, was considerably stretched out towards his limb, which gave the planet nearly the form of a pear; and even after the separation of the limbs Venus was twelve or nine seconds before she resumed her rotundity.'

The following cases, with their accompanying illustrations, serve at once to indicate the nature and suggest the explanation of the peculiar appearances presented by Venus when nearly at internal contact.

Fig. 7 represents the appearance presented by Venus as observed by Mayer at St. Petersburg, in 1769. A reference to Plate V will show that at St. Petersburg the sun was almost upon the horizon at the moment of ingress, and close to the horizon at the moment of egress. There can be no doubt that the distorted appearance of Venus is due to atmospheric disturbances, such as are always recognisable when the sun is observed low down. I may remark that fig. 7 corresponds precisely to what I observed when examining the artificial transit of Venus as arranged at Washington, on a morning when the atmosphere was unusually disturbed. The American astronomers

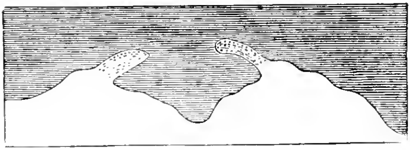


Fig. 7.—Appearance presented by Venus at Internal Contact, as observed by Mayer.

consider that the corresponding arrangements at Greenwich are not so good as their own, because the distance between the observer and the artificial 'Sun and Venus' is not great enough to permit the study of these atmospheric effects. We see clearly enough from Mayer's observation that such effects, though they would not be nearly so great with the sun even moderately raised (say 10°) above the horizon, must always be taken into account. The edge of the sun even at a considerable height is always rippled by the effects of atmospheric undulations. So also necessarily must the outline of Venus be rippled, and it is the contact of two rippling outlines, not of two sharply defined discs, that the astronomer is called upon to observe.

The next picture (fig. 8) is from a drawing by Bayley at Nord Cap. In this case the sun was raised about 10° from the horizon, but the blurred outline given to the sun indicates the existence of imperfect atmospheric conditions, and we may partly attribute to this cause the wideness of the connecting ligament when contact was actually established.

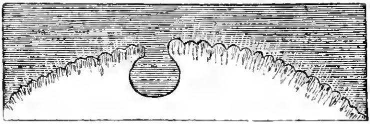


Fig. 8.—Contact of Venus, as observed by Bayley at Nord Cap.

Fig. 9 is from a drawing by Hirst, who observed the ingress at Greenwich; while fig. 10 shows how

Venus appeared to Bevis, who observed at Kew under nearly the same conditions.

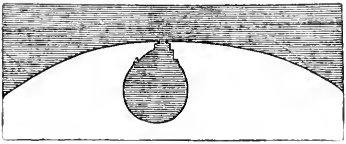
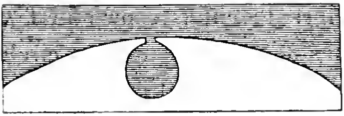


Fig. 9.—The 'Black Drop,' as observed by Hirst.

There has been much discussion as to the cause of the 'black drop,' and in some instances considerable energy has been evinced in the attempt to show that this or that cause is the true one. It appears probable that the phenomenon is occasioned by the combination of several causes, and is widely variable in its extent. The general cause—by which I mean the resultant of the various causes in operation—is manifestly an apparent extension of the sun's disc, and an apparent contraction of the disc of Venus. Suppose, for instance, that the arc s s′v′ (fig. 11) represents part of the true outline of the sun, then this outline appears shifted outside its true place, or to the position indicated by the boundary between light and shade in the figure; and the apparent outline of Venus is shifted



10.—The 'Black Drop,' as observed by Bevis.

from s′v v′, its true position, to that shown by the outline of the black disc. Supposing this shifting of the outline to be uniform, and to continue unchanged in extent, as Venus gradually passes on to the sun's face, it is clear that at the moment of true contact, when the real outlines touch, as shown in fig. 12, the apparent outlines will belong to two circles which are far from touching. But at the actual point of contact, where the widening of one outline and the contraction of the other cannot be supposed to act, there will still remain a fine black ligament. Under less perfect conditions this moment of true contact would not be attained, and instead of a fine ligament being seen just before Venus separated from the sun's edge, a wider ligament would be observed.

Fig. 11. Fig. 12.

Illustrating the Formation of the 'Black Drop.'

Thus far I have only indicated the general cause. And it may be said that this general cause is demonstrated by the observed effects. But we must now consider how this general cause is itself brought about; and herein lies the difficulty of the matter, whether regarded as a problem or considered with reference to the practical mastery of this occasion of error.

First, we have the rippling I have spoken of. Taking any point on the outline of the sun's disc or of Venus's, that point is swayed backwards and forwards across its true position by the effect of atmospheric undulation, the range of oscillation being greater or less according as the atmosphere is more or less perturbed, and as the sun is observed nearer to or farther from the horizon. A moment's consideration will show that the effect of such oscillations, operating all round both discs, must be to cause the sun's disc to cover (on the whole) a larger space than it should, while the disc of Venus covers a less space than it should. For there is a certain fringe of space all round both discs which is partially illuminated by these oscillatory movements, and this partial illumination extends the sun's disc outwards and contracts that of Venus. Probably this cause has but a small share in producing the general effect, except when the sun is low down.

Secondly, there is the optical effect caused by the fact that the image of a bright point is not itself a point. And here we have three causes in operation, which we may consider together. First, in the most perfect telescope the image of a point is what is called the 'circle of least confusion' between the two linear or almost linear) foci. Secondly, diffraction affects the dimensions of the focal image of a point of light. And thirdly, if the telescope is defective, spherical aberration may operate so as seriously to affect the definition. All these causes combine to alter the image of each point of the outlines of the sun and Venus into a small disc of light instead of a point. The result necessarily is that the outlines extend beyond the true boundary of light and dark; that is, the disc of the sun is enlarged and that of Venus is contracted.

Thirdly, there is a cause which might, perhaps, have been combined with the last—the qualities of the eye regarded as an optical instrument; for the image of a point on the retina is not a point but a minute circle, even when the object is viewed directly.

Fourthly, there is the effect called irradiation, by which the apparent size of a bright object is enlarged. This effect will be greater or less according as the contrast between the bright object and the dark background on which it is projected is greater or less. Moreover, it appears that irradiation not only differs in amount with different observers, but varies even with the same observer at different times. Nay, its amount varies from moment to moment with the varying mental effort made by the observer to ascertain, more or less exactly, the true outline of the observed object.

We cannot wonder if the observations of the transit of 1761, affected as they were by peculiarities of appearance resulting from these various causes, for the operation of which observers were not on that occasion prepared, led to no trustworthy results.

CHAPTER III.

THE TRANSIT OF 1769.

THE general impression among astronomers, after the observations of 1761 had been discussed, was that too much reliance had been placed on Delisle's method. 'Experience,' wrote J. D. Cassini, later, in his 'Histoire du Passage de 1769,' 'is our chief instructor; the fruit of its lessons indemnifies us for the value of the years they cost us. The principal end had been missed, in 1761, for want of observations in places where the durations differed sufficiently. It was essential not to experience a second time the same disadvantage.'

Among the first statements published respecting the transit of 1769 was that by the ingenious Ferguson, who wrote as follows in 1762: 'On the 3rd of June, in the year 1769, Venus will again pass over the sun's disc, in such a manner as to afford a much easier and better method of investigating the sun's parallax than her transit in the year 1761 has done. But no part of Britain will be proper for observing that transit,[1]

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[1] This was an error, due to Ferguson's reliance on Halley's tables; not, I need hardly say, the tables by which Halley had arrived at his

so as to deduce anything with respect to the sun's parallax from it, because it will begin but a little before sunset, and will be quite over before two o'clock next morning. The apparent time of conjunction of the sun and Venus, according to Dr. Halley's tables, will be at thirteen minutes past ten o'clock at London, at which time the geocentric latitude of Venus will be full ten minutes of a degree north from the sun's centre; and therefore, as seen from the northern parts of the earth, Venus will be considerably depressed by a parallax of latitude on the sun's disc; on which account the visible duration of the transit will be lengthened; and in the southern parts of the earth she will be elevated by a parallax of latitude on the sun, which will shorten the visible duration of the transit with respect to its duration as supposed to be seen from the earth's centre; to both which affections of duration the parallaxes of longitude will also conspire. So that every advantage which Dr. Halley expected from the late transit will be found in this, without the least difficulty or embarrassment. It is, therefore, to be hoped that neither cost nor labour will be spared in duly observing this transit, especially as there will not be such another opportunity again in less than 105 years afterwards.'

Ferguson also showed accurately the places where advantage could be best taken of Halley's method: 'The most proper places for observing the transit in

incorrect ideas respecting the circumstances of the earlier transit, but those which Halley had formed subsequently.

the year 1769 are in the northern parts of Lapland, and the Solomon Isles in the Great South Sea, at the former of which the visible duration between the two internal contacts will be at least twenty-two minutes greater than at the latter, even though the sun's parallax should not be quite 9″. If it be 9″ (which is the quantity I had assumed in a delineation of this transit which I gave in to the Royal Society before I had heard what Mr. Short had made it from the observations of the late transit), the difference of the visible durations, as seen in Lapland and in the Solomon Isles, will be as expressed in that delineation; and if the sun's parallax be less than 9″ (as I now have very good reason to believe it is) the difference of durations will be less accordingly.'

Later, Hornsby in England, and De Lalande and Pingré in France, discussed very carefully the circumstances of the transit of 1769. De Lalande, in 1764, illustrated the conditions of the transit of 1769 by a projection of the earth planned like that which Delisle had made for the transit of 1761. The tables of Cassini formed the basis of the calculations made by these astronomers; and as Cassini had had the advantage of later observations of Venus, his tables were necessarily more accurate than those which Halley had completed earlier in the century.

The circumstances of the transit of 1769 in different parts of the earth can be inferred from what is shown in Plate V. Here the arcs A I B and A′ I′ B′ separate the dark and light hemispheres of the earth at the beginning of the transit, while the arcs C E D and C′ E′ D′ separate the dark and light hemispheres at the end of the transit. Thus the beginning was visible at all places on the hemisphere formed by combining the sections A I B D and A′ I′ B′ D′; and the end of the transit was visible at all places on the hemisphere formed by combining the sections C E D A and C′ E′ D′ A′. The whole of the transit was visible over the spaces D *e i* A and D′ *m′* A′; but within the space *i m e*, though the beginning and end of the transit were seen, the progress of the transit was not *wholly* visible. No part of the transit was visible over the spaces B *m* C and B′ *i′ e′* C′; but within the space *i′ m′ e′*, though neither the beginning nor the end were visible, the progress of the transit was partially visible. At all points of the arcs A I B and A′ I′ B′ the ingress occurred with the sun on the horizon; but whereas the sun was rising for the arcs A *i* and A′ *i′*, he was setting for the arcs B *i* and B′ *i′*. At all points of the arcs C E D and C′ E′ D′ the egress occurred with the sun on the horizon; but whereas the sun was rising for the arcs C *e* and C′ *e′*, he was setting for the arcs D *e* and D′ *e′*. At the points *m* and *m′* the sun was on the horizon both at ingress and at egress; but whereas the whole transit, except ingress and egress, took place during the night at *m*, it took place during the day at *m′*. All that has here been said has related to the passage of Venus's centre.

The point I was that where ingress occurred earliest,[1] the point I′ being that where ingress occurred latest. It was around these points that observers of ingress by Delisle's method were to be placed, on that side of the arcs A B and on A′ B′ where the sun would be above the horizon at the time of ingress. We see that Great Britain was admirably placed for observing accelerated ingress, Greenwich being almost as well placed as any station could possibly be, though having the sun rather low (and unfortunately it appeared from Halley's tables as though the sun would be still lower). At Greenwich sunset was approaching when transit began. I′ was in a little known part of the Southern Seas.

The point E′, where egress occurred earliest, was, like I′, placed in a part of the Southern Seas about which little was at that time known. E, where egress occurred latest, was so placed that the whole of

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[1] Encke, in 1822, found the following elements for the transit of 1769. I quote these, like the elements for 1761, from M. Dubois' 'Les Passages de Vénus sur le disque Solaire':—

	h.	m.	s.	
Ingress of Venus's centre	7	26	54·5	Greenwich apparent solar time.
Middle of the transit	10	27	20·8	
Egress of the centre	13	27	51·3	
Duration of the transit	6	0	56·8	
Least distance of centres	0	10	8·1	

	Latitude		Longitude	
	°	′	°	′
Pole of accelerated ingress	49	33 N	7	23 E
,, ,, retarded ,,	49	33 S	172	37 W
,, ,, accelerated egress	22	30 S	122	46 W
,, ,, retarded ,,	22	30 N	57	14 E
,, ,, shortened durations	38	37 S	143	2 W
,, ,, lengthened ,,	38	37 N	39	58 E

India and the region between the north-west of India and the Sea of Aral was suitable for observing this phase.

But chief interest was attached, as I have said, to the application of Halley's method. The Halleyan poles were at H and H′, these being respectively the points where, in a geometrical sense (that is, without taking into account the actual visibility of ingress or egress), the transit would be respectively most lengthened and most shortened. H, we see, lay in a region whence no part of the transit could be seen, and the point *m* was the nearest to H where both the beginning and end would be visible, but with the sun upon the horizon. The space *i m e* was that which presented the most promising conditions, except that the sun would be low within this space, both at ingress and at egress (passing below the horizon for a greater or less portion of the progress of the transit). Wardhuus, in Lapland, close to this region, was selected for the most northerly Halleyan stations; and as the polar regions could not be occupied, the stations next in order of value were necessarily those lying on the opposite side of the arctic circle, from Kamschatka, through Alaska, &c., round to Hudson's Bay. These, however, were too far away from H to be of great value as Halleyan stations, while their great distance from I and E prevented them from having any special value as Delislean stations. The southern Halleyan pole H′ was in the same unknown quarter of the Southern Seas in which I′ and E′ are seen to lie. In fact, the whole region around H′ was of extreme importance for the observation of the transit of 1769, since any station placed there would not only be excellent for Halley's method, but also for Delisle's, both as respects retarded ingress and accelerated egress.

In passing, let it be noted how the superiority of the second transit of a pair (in general) shows itself by the positions of H and H′ in Plates IV. and V. respectively. In Plate IV. we see that H and *m* lie on opposite sides of the north pole, H′ and *m*′ on opposite sides of the south pole; whereas in Plate V. we see that H and *m* are on the same side of the north pole, H′ and *m*′ on the same side of the south pole. Now, necessarily one Halleyan pole lies in the region whence no part of the transit can be seen, and we see that in such a case as that illustrated by Plate V. the point *m* indicates how near that particular Halleyan pole H can be approached without losing either the beginning or end of the transit; whereas in the case illustrated by Plate IV., *m*′, the point of nearest available approach, lies very much farther away from the corresponding Halleyan pole H′. Still, it is to be noticed that, even in the case of a transit like that of 1769 (Plate V.), the really effective use of Halley's method requires that a station should be reached near the space corresponding to *e m i*. If this cannot be arranged, the stations next in order of value are those lying on the farther side of the arctic or antarctic circle (as the case may be), and such stations will commonly not be much better than one near m' for the transit of 1761 (Plate IV.), and may be even far inferior to stations available in the case of such a first transit as that of 1874 (Plate VI.). This is, in fact, the reason why Halley's method fails totally in 1882 (see Plate VII.), though this is the second transit of a pair.

The actual operations for viewing the transit of 1769 were carried out on a widely extended scale. Preparations were made for sending observers to the South Sea, California, Mexico, Lapland and Kamschatka. The King of Denmark invited Father Hell, the eminent German astronomer, to observe the transit at Wardhuus, in Lapland, and thither Hell betook himself with Borgrewing, the Danish astronomer. They arrived in the autumn of 1768, and passed the winter in that desolate region. Chappe d'Auteroche was selected by the French Academy to observe the transit from the Solomon Isles, in the South Sea; but, says M. Dubois, 'the South Sea at that epoch was under the rule of Spain, and it was only possible to visit those seas in a Spanish vessel, and with the permission of the Court of Spain. The Spanish Government refused such permission, but gave Chappe leave to embark in the Spanish fleet then about to sail for Western America.' Chappe eventually observed at St. Joseph, in California.

'England,' says M. Dubois, 'did not wait for permission from Spain to send an astronomer to observe the transit from the South Sea.' The following account, taken from 'Cook's Voyages,' describes the preparations made for the journey :—

'It having been long before calculated that the planet Venus would pass over the sun's disc in 1769. a phenomenon of great importance to astronomy, and which had engaged the attention of men of science, it was judged that the most proper place for observing this phenomenon would be either at the Marquesas or at one of those islands to which Tasman had given the several appellations of Amsterdam, Rotterdam, and Middleburg, but which are now better known under the general name of the Friendly Islands. This being a matter of so much importance in the science of astronomy, the Royal Society, with that laudable zeal they have ever shown for its advancement, presented a memorial to his Majesty at the beginning of the previous year, requesting among other things that a vessel might be fitted out, at the expense of the Government, to convey proper persons to observe this transit at one of the places already mentioned. The petition being readily complied with, and orders having been given by the Admiralty to provide a vessel for that purpose, on April 3, Mr. Stephens, the Secretary to the Board, informed the Society that everything was progressing according to their wishes.

'Mr. Dalrymple was originally fixed upon to superintend this expedition : a man eminent in science, a member of the Royal Society, and who had already greatly distinguished himself respecting the geography of the Southern Ocean. As this gentleman had been regularly bred to the sea, he insisted (very properly too) on having a brevet commission, as captain of the vessel, before he would undertake the employment. Sir Edward Hawke (afterwards Lord Hawke, a naval officer, and not a civilian), who then presided at the Admiralty, violently opposed this measure; and being pressed on the subject, declared that nothing would induce him to give his sanction to such a commission.

'Both parties were inflexible, and it was therefore thought expedient to look out for some other person to conduct the expedition. Accordingly, Mr. Stephens, having recommended Lieutenant Cook, and this recommendation having been strengthened by the testimony of Sir Hugh Palliser, who was well acquainted with Cook's merit and abilities for the discharge of this office, he was appointed to this distinguished post by the Lords Commissioners, and promoted to the rank of Lieutenant of the Royal Navy on May 25, 1768. He was now, be it remembered, close upon forty years of age.

'This appointment having taken place, Sir Hugh Palliser was commissioned to provide a vessel adapted for such a voyage. After examining a great number then lying in the Thames, in conjunction with Cook, of whose judgment he entertained the highest opinion, they at last fixed upon the 'Endeavour,' a barque of 370 tons, which had been built for the coal-trade.

'In the interim, Captain Wallis having returned from his voyage round the world, and having signified to the Royal Society that Port Royal Harbour, in

King George's Island, now called Otaheite, would be the most convenient place for observing the transit, his opinion was adopted, and the observers were ordered to repair thither.

'Mr. Charles Green, the coadjutor of Dr. Bradley, the Astronomer Royal, was nominated to assist Captain Cook in conducting the astronomical part of the undertaking; and he was accompanied also by Joseph Banks, Esq. (afterwards Sir Joseph, the President of the Royal Society). This friend of science possessed at an early period of life an opulent fortune, and being zealous to apply it to the best ends, embarked on this tedious and hazardous enterprise, animated by the wish of improving himself and enlarging the bounds of knowledge. He took two draughtsmen with him, and had likewise a secretary and four servants in his retinue.

'Dr. Solander, an ingenious and learned Swede, who had been appointed one of the librarians in the British Museum, and who was particularly skilled as a disciple of Linnæus, and distinguished in his knowledge of natural history, likewise joined the expedition. Possessed with the enthusiasm with which Linnæus inspired his disciples, he braved danger in the prosecution of his favourite studies; and being a man of erudition and capability, he added no small *éclat* to the voyage in which he had embarked.

'Though the principal intention of this expedition was to observe the transit of Venus, it was thought proper to comprehend other objects as well. Captain

Cook was therefore directed, after he had accomplished his main business, to proceed in making further discoveries in the South Seas, which now began to be explored with uncommon resolution.'

The expedition sailed from Deptford on July 30, 1768, and on August 13 anchored in Plymouth Sound, from which after a few days' stay they proceeded to sea. It was not until April 10 that they saw Otaheite. 'On the 10th,' says the narrative, 'upon their looking out for the island to which they were destined they saw land ahead. The next morning it appeared very high and mountainous, and it was known to be King George the Third's Island, so named by Captain Wallis, but by the natives called Otaheite.'

In May they 'began to make preparations for observing the transit of Venus; and from the hints which Captain Cook had received from the Royal Society, he sent out two parties to make observations from different spots, that in case they failed at Otaheite they might succeed elsewhere. They employed themselves in preparing their instruments, and giving instructions in the use of them. On Thursday, June 1 (the next Saturday being the day of the transit), they sent the long-boat to Eimayo, having on board Mr. Gore, Mr. Monkhouse, and Mr. Sporing, a friend of Mr. Banks, each furnished with necessary instruments by Mr. Green. Mr. Banks and several of the Indians went out with this party. Others were despatched to find out a convenient spot at such a distance from their principal station as might suit their purpose.

Those who went to Eimayo in the long-boat, after rowing the best part of the night, by the help of some Indians on board a canoe which they hailed, found a proper situation for their observatory upon a rock, where they fixed their tents, and prepared the apparatus for the following day's observation. On Saturday, June 3, as soon as it was light, Mr. Banks left them to go to the island for fresh provisions. As he was trading with the natives who belonged to Tarras the king of the island arrived, with his sister, whose name was Nuna, in order to pay him a visit. . . . Mr. Banks returned to the observatory with his visitors, and showed them the transit of the planet Venus over the sun's disc, informing them that he and his companions had come from their own country solely to view it in that situation. Both the parties which were sent out made their observations with great success. They nevertheless differed in the accounts of the times of transits more than might have been imagined.' In Captain Cook's journal, the following account is given: 'The day proved as favourable to our purpose as we could wish; not a cloud was to be seen the whole day, and the air was perfectly clear; so that we had every advantage in observing the whole of the passage of the planet Venus over the sun's disc. We very distinctly saw an atmosphere, or dusky shade, round the body of the planet, which very much disturbed the times of the contact, particularly the two internal ones. It was nearly calm the whole day, and the thermometer, exposed to the sun about the middle of the day, rose to a degree of heat we have not before met with.'

Chappe was specially fortunate at St. Joseph. His observation has given rise to a good deal of controversy, with regard to its bearing on the question of the solar parallax. Powalky and others consider that Chappe's observation of the internal contact at egress was an observation of real contact, not apparent contact; Stone maintains the contrary. My attention was specially directed to this point by Newcomb, of Washington, U.S., and I must confess that Chappe's narrative seems to me unquestionably to bear the interpretation given to it by Powalky, with whom Newcomb agrees. Let the reader judge, remembering that real contact means the formation of the black drop or of the pear-shaped figure described at page 57; so that at total ingress real contact is later than apparent, while the reverse is the case at egress. Chappe writes as follows:—'At the total ingress I observed very distinctly the second phenomenon, which had been noticed by the greater part of the observers in 1761. The edge of the disc of Venus lengthened itself, as if it had been attracted by the sun. I did not observe, for the instant of total ingress, the instant when the edge of Venus commenced to extend itself; but, not being able to doubt that this black point was not part of the opaque body of Venus, I observed the moment when it ended ('où il était à sa fin') in such sort that the total ingress could not have occurred earlier, though perhaps later by two or three seconds. The black point was a little less dark than the rest of Venus; I think it is the same phenomenon which I had observed at Tobolsk in 1761. . . . At the second internal contact' (that is, internal contact at egress), 'the sun was undulating, as was Venus also, which rendered the observation very difficult. At this contact Venus elongated herself more considerably than in the morning, in approaching suddenly the edge of the sun.' It seems clear that Chappe here witnessed that sudden leap to the sun's edge at egress which is the counterpart of the sudden leap from the sun's edge at ingress; and that if the contact differed at all from the contact at ingress, it was in the fact that a longer leap was made, in other words, that he caught an earlier phase at ingress, which would correspond of course to a later phase at egress. As Chappe says himself that real contact at ingress might have been two or three seconds later, but certainly not earlier, we see that the contact he observed at egress corresponded even more closely with what he regarded as real contact,—that is, the moment of the leap by which the black drop is formed and broken. Yet Mr. Stone considers that at egress Chappe missed the real contact and observed the later phase of apparent contact.[1]

Le Gentil experienced in 1769 the culmination of

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[1] Here and at pp. 90–92, I retract the views I expressed in my 'Sun,' and in reply to Prof. Newcomb's general criticism on my account of Stone's work. So soon as we met, and he described his objections in detail, I recognised their force. The Astronomical Society had, in fact, pronounced so decisively in favour of Stone's treatment of the transit of 1769, that I was not prepared to find errors so serious in it.

his misfortune. With a persistent courage worthy of better success he determined, after his failure in 1761, to return to Pondicherry as soon as an opportunity presented itself, and to await there during eight years the transit of 1769. Dubois remarks that Le Gentil usefully employed those years in studying the astronomy of the Brahmins, on which subject he published an interesting work upon his return to France. But the object he had specially in view was unfortunately not attained. 'On June 3, 1769,' says Dubois, 'at the moment when this indefatigable observer was preparing to observe the transit, a vexatious cloud covered the sun, and caused the unhappy Le Gentil to lose the fruit of his patience and of his efforts.' Pondicherry would have been a useful station for observing the retarded egress, as we see from Plate V.

Pingré, who had observed the transit of 1761 at Rodriguez, was sent to observe the transit from a French station in the island of St. Domingo.

Although the observations made in 1769 were on the whole much more satisfactory than those which had been made in 1761, yet there was much to throw doubt on any determination of the sun's distance based even on the later transit. We have seen already that the peculiar distortion of Venus, illustrated in pp. 61–63, was presented in a marked degree in 1769; but even more unpromising was the observed difference in time between the moments of real and apparent contact. It is only necessary, as M. Dubois points out, to consider the difference recognised by those observers who noted the two phases to see how largely the accuracy of the deduced solar distance must be affected by this cause.

Wales, at Hudson's Bay, using a telescope two feet long, magnifying 120 times, found a difference of 24 seconds between the real and apparent contacts at egress. Green, at Otaheite, found a difference of 40 seconds at ingress and 48 seconds at egress. Cook, at the same station, found the difference 60 seconds at ingress and 32 seconds at egress. Yet these two observers used two similar telescopes, magnifying 140 times. Maskelyne, at Greenwich, using a telescope magnifying 140 times, found the difference 52 seconds; while Horsley, at the same station, with an achromatic telescope, 10 feet in length, magnifying 50 times, found the difference to be 63 seconds. Maskelyne remarks that the difference was greater than he had expected, considering that the telescopes were all nearly of the same quality, except a reflector of six feet used by Hitchins. The superiority of this instrument appeared to Maskelyne to account for the difference of 26 seconds, by which interval Hitchins observed the internal contact earlier than Maskelyne. Hornsby, at Oxford, used an achromatic telescope of 7½ feet, magnifying ninety times, and found the difference to be 57½ seconds; while Schuckberg, also observing at Oxford, found a difference of 69 seconds between the real and apparent contacts. An unknown observer at Caen, using a very small telescope, found the enormous difference of fully 150 seconds! Wilke, at Stockholm also using a very small telescope, estimated the difference at 43 seconds. Lastly, Euler, observing at Orsk, with a telescope 12 feet in length, noted for the instants of contact two epochs differing by 50 seconds.

When we consider these wide and widely varying differences among observers who observed both kinds of contacts, we cannot wonder if considerable differences of absolute time were noted between observations of the same contact by different observers either at the same stations or at stations near enough for instituting a comparison. Thus, Le Monnier and De Chabert, at St. Hubert, noted instances of contact differing 36 seconds from each other; while between Duval le Roy and De Verdun, at Brest, there was a difference of 30 seconds.

It is well remarked by Dubois that observations of external contact at ingress can have no value. He adds that observations of external contact at egress are somewhat more reliable; but it must be very difficult to distinguish the moment when the solar limb resumes an exactly circular shape. Accordingly the fourteen exterior contacts noted by different observers could have no real value. Yet it is worthy of remark that the difference between moments of external contact observed at St. Petersburg by Mayer and Stahl amounted only to 27 seconds; while at Gurief the difference between two such observations amounted to 28 seconds. So that, as Newcomb has remarked of the observations made during the transit of Mercury in November 1868, it would seem as though the errors in the estimated instant of an external contact might be expected to be of the same order as those affecting the estimated instant of an internal contact.

Dubois tells us that upwards of two hundred memoirs were sent to the Academy of Sciences on the value of the solar parallax deducible from the observations made in 1769. How many were sent to the Royal Society I do not know; but probably as many as four hundred were sent to the different learned bodies of Europe.

A comparison of the results obtained by the most competent computers showed that the observations of 1769 were much more valuable on the whole than those of 1761; for, whereas the results obtained in 1761 ranged in value between 8″·5 and 10″·6, we find the following five results selected as those most carefully calculated on the basis of the observations of 1769:—

			″
De Lalande	fixed the	parallax at	8·50
Fr. Hell	,,	,,	8·70
Hornsby	,,	,,	8·78
Euler	,,	,,	8·82
Pingré	,,	,,	8·88

The solar distances corresponding to the parallaxes 8″·50 and 8″·88 are respectively 96,162,840 miles and 92,049,650 miles.

It is somewhat singular that, notwithstanding the clearest evidence of a cause of uncertainty sufficing to account for such differences as the above table presents, Lalande and Pingré, who had obtained the most widely different results, were both quite confident of the accuracy of the values they had deduced. Lalande says in his memoir, that regarding the whole series of observations of 1769, the solar parallax is incontestably 8″·5; while Pingré says in reply, 'of two things one: either no result at all can be deduced from the transit of 1769, or it must be admitted that the value of the solar parallax is very close indeed to 8″·8 (est à très-peu près de 8″·8).'

In his first memoir, in which the above tabulated value, 8″·82, was given, Euler had not taken into account the observations made at Otaheite by Green and at St. Joseph, in California, by Chappe. Going over his work afresh, and introducing these observations, he deduced the parallax 8″·68. Dionis du Séjour, employing only observations of duration, and combining the transits of 1761 and 1769, deduced the value 8″·84. But in his 'Traité Analytique des Mouvements Apparents des Corps Célestes' he adopts as the final result of his calculations the solar parallax 8″·8128.

It is worthy of notice that when chief reliance was placed on observations made at Halleyan stations o the first class, the value of the parallax approached more nearly to that now recognised as probably the more correct. Thus, combining observations made at Otaheite with Father Hell's observations at Wardhuus, De Lalande obtained the value 8″·72, and yet larger values when Hell's observations were combined with other observations. Yet, as we have seen, De Lalande adopted 8″·5 as the best mean value of the solar parallax.

Doubts, indeed, were thrown upon Father Hell's observations, on account of corrections which had been made in his MS. notes of the phenomena, (and partly, also, because of the known fact that he alone of all the observers of the transit recognised no distinction between real and apparent contacts). The idea that Hell's records were forged was thrown out by the Astronomer Royal. But such a suspicion need hardly be seriously considered. Not only is nothing known about Fr. Hell which for a moment justifies the supposition that he could be guilty of the act charged to him, but we know now that his observations accord better with the latest estimates of the parallax than those of other observers.

Encke in 1824 published an analysis of the observations of the transit of 1769, from which he deduced for the solar parallax the value 8″·6030. By combining the observations of both transits he deduced that value 8″·5776 (corresponding to a solar distance of 95,274,000 miles) which for more than a quarter of a century thereafter maintained its ground in treatises on astronomy.

But about the year 1850 it began to be recognised that the sun's distance had been over-estimated. Various methods of determining the solar parallax, inferior singly to the observation of transits of Venus, but collectively superior—and superior, moreover, because of the greater accuracy with which (owing to the improvement in instruments of precision) they could be applied—concurred in showing that the sun's distance was less than had been supposed by at least three millions of miles. The consideration of these methods in detail would occupy more space than is here convenient. The reader will find them fully described in the second chapter of my treatise on the sun. In this place let the following summary suffice:—

In 1854 Hansen announced that by a method based on observations of the moon's motions he had deduced the parallax 8″·9159, corresponding to a distance of 91,659,000 miles. Leverrier, from the careful study of the sun's apparent motions, as affected by the earth's monthly revolution around the common centre of gravity of herself and the moon, deduced a solar parallax of 8″·95, corresponding to a distance of 91,330,000 miles. Prof. Newcomb, of Washington, U.S., obtained by the same method the parallax 8″·84, distance 92,500,000 miles. From observations of Mars when at his nearest to the earth Prof. Newcomb deduced the parallax 8″·85, corresponding to a distance of 92,300,000 miles. Stone, formerly of Greenwich, obtained by this method the distance 91,400,000 miles; while Winnecke deduced the distance 91,200,000 miles. Foucault, measuring the velocity of light by means of a rapidly revolving mirror (a plan devised by Wheatstone), and comparing the value so obtained with that inferred from the observation of the eclipses of Jupiter's satellites and the aberration of light, deduced the solar parallax 8″·86, corresponding to a distance of 92,100,000 miles. From the study of those planetary perturbations which depend on the relative masses of the earth and the other planets Leverrier deduced the value 8″·859 for the parallax, or 92,110,000 miles for the sun's distance. It will be seen that the values thus obtained indicate a solar parallax of 8″·89, corresponding to a distance of about 91,950,000 miles. The limits of probable error are considerable, however, and we scarcely know more at present than that the solar parallax almost certainly lies between the values 8″·82 and 8″·96, corresponding to the distances 92,676,000 miles and 91,228,000 miles.

As soon as it became clearly recognised that Encke's estimate of the sun's distance from observations of the transit of 1769 was considerably in error, doubt necessarily fell upon the method itself which had till then been regarded as the most satisfactory for determining the sun's distance. Efforts, however, were made to restore the credit of the method by a re-examination of the observations made in 1769. These efforts have been regarded by many, especially in this country, as successful; but it must be confessed the investigation has shown us rather how the error crept in than how it can be avoided in future applications of the method. This will appear when we consider the nature of the researches by which astronomers have sought to restore the waning credit of the observations of 1769.

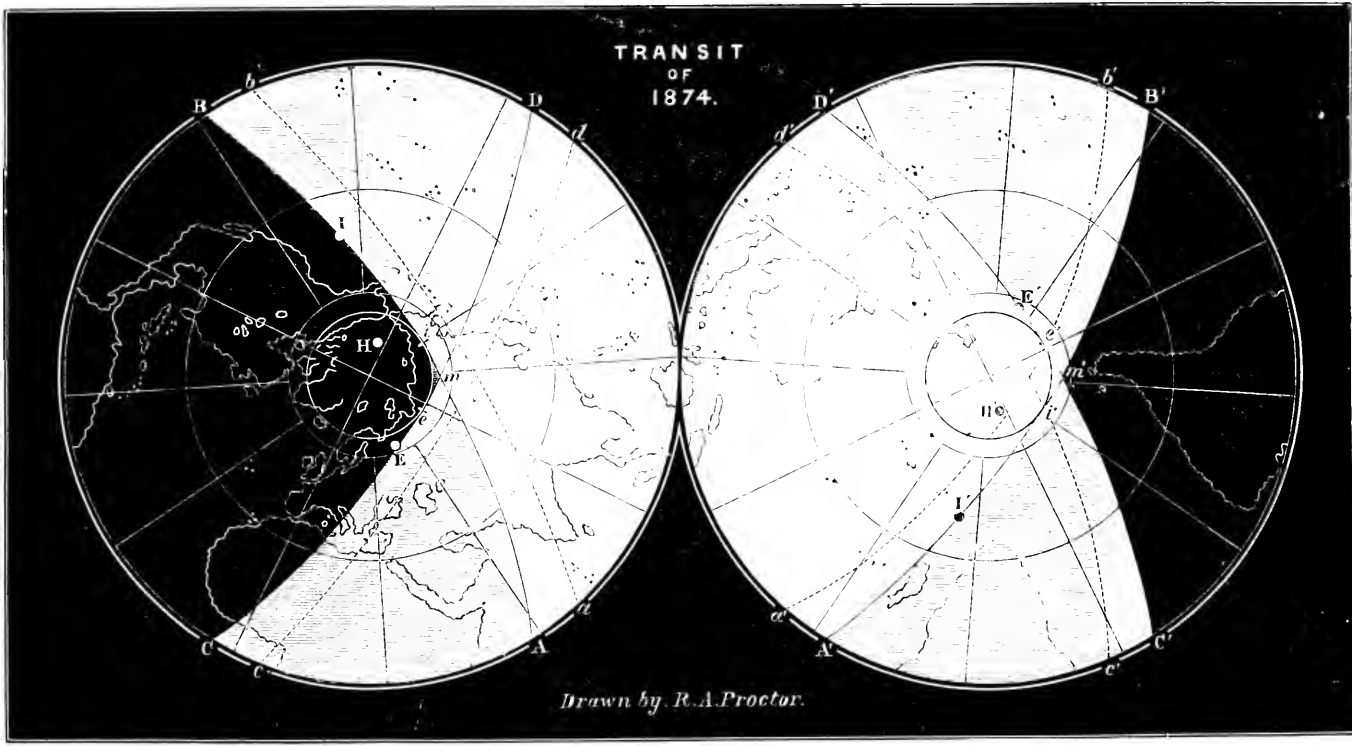
Powalky in 1864 discussed forty-four observations, made at nineteen stations, the latitude of which seemed to him satisfactorily determined. He dismissed seventeen of these observations, and treated six of the remaining twenty-seven as of inferior worth, giving to them only half the weight assigned to the other twenty-one. The observations thus retained were made at only thirteen stations. Amongst the observations were nine external contacts, four at ingress and five at egress. Powalky gives no sufficient reason for some of the selections made by him (between conflicting observations made at stations in the same regions), nor for regarding as real contacts some observations which were not described with sufficient exactness to justify that interpretation. On the whole, it seems impossible to regard his conclusion as satisfactorily established. All he can be said to have proved is that amongst the observations made in 1769 it is possible to select several which, when combined, give a value of the solar parallax according fairly with the estimate recently adopted in preference to Encke's.

The investigation published by Mr. Stone, of Greenwich, in 1868, has been regarded as more trustworthy; but it does not appear that those who have expressed approval of it had critically examined his memoir on the subject. When Sir John Herschel described Stone's work as removing the reproach from astronomy which had fallen upon the science in consequence of the large error detected in the estimate of the sun's distance, he appears to have taken Stone's results for granted, and not only so, but to have mistaken their real significance. I followed him in my treatise on the sun, being partly influenced by the fact that the Astronomical Society had adopted Stone's conclusions. But my attention having been directed by Professor Newcomb, of America, to the slightness of the examination given to Stone's memoir by those who had accepted its results, I have been led to examine it for myself, and I am obliged to admit that it has much less weight than some in this country have supposed.

What Stone has done has been simply this. He has endeavoured (as others had done) to ascertain from the account given by each observer, whether real or apparent contact was observed. And he introduced in the equations of condition a constant correction (seventeen seconds) for the difference in time between the two contacts. Now, this constant is inferred from the equations themselves whence the parallax deduced by Stone, 8″·91, is obtained; and his analysis really amounts to the distribution of the disposable errors affecting the observations of contact, in such sort that a part goes to change the parallax from Encke's value to 8″91, and the remainder to form the constant correction between real and apparent contacts. This would render the result unreliable, even if we had reason to believe that the correct time-difference between real and apparent contacts in any given transit really had a nearly constant value, and that value not far from seventeen seconds. Knowing as we do from the accounts of the observers themselves that the difference varied greatly with the varying circumstances under which the observations were made, and *always largely exceeded seventeen seconds,* it seems quite impossible to adopt Mr. Stone's method as trustworthy. We cannot, therefore, wonder that Continental and American astronomers have, by common consent, declined to accept Mr. Stone's results as having much weight, or indeed as proving anything except what had already been ascertained—the fact, namely, that the observations made in 1769 afford but unsatisfactory evidence respecting the sun's distance.

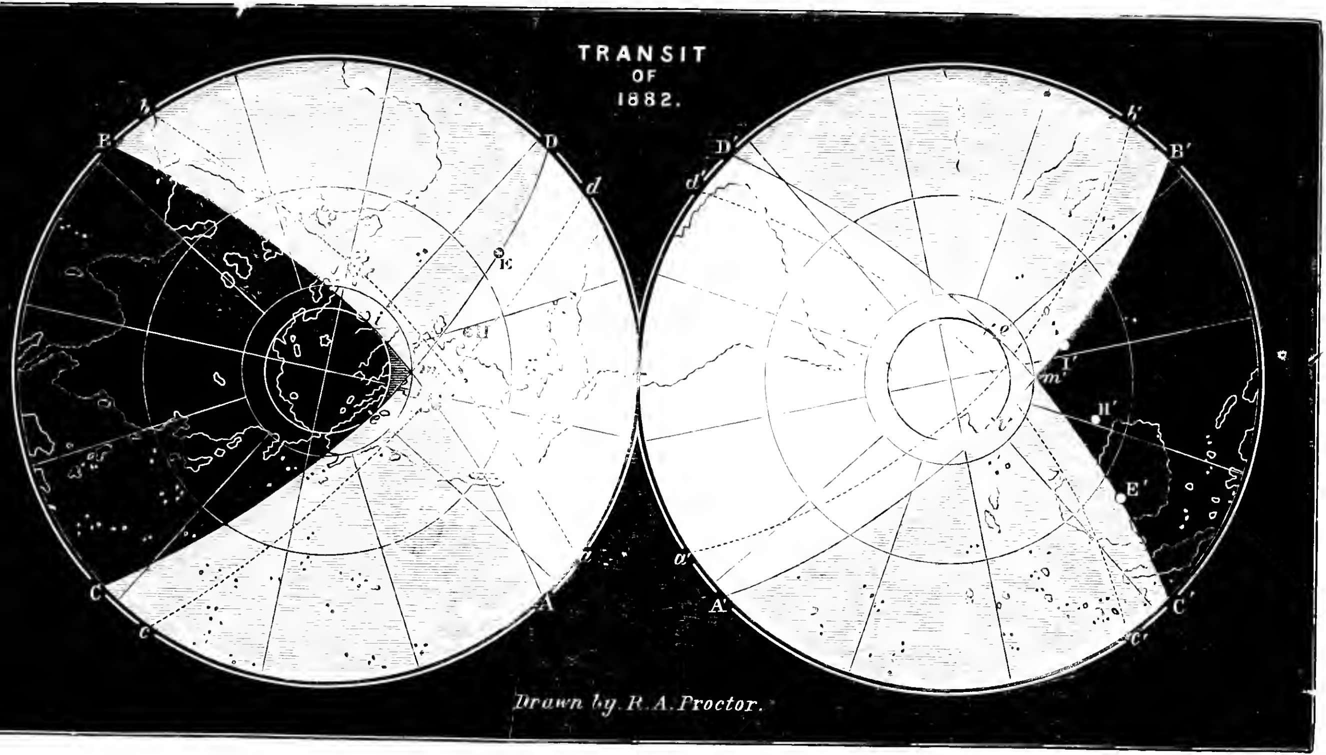
But the imperfect nature of the observations made in 1761 and 1769 can be sufficiently explained without attributing inferiority to the method of determining the sun's distance in pursuance of which the observations were made. It cannot be doubted that the measurement of the sun's distance resulting from those observations was more trustworthy than any which could have been obtained at that time by other methods. We have learned to apply other methods so much more accurately than they could have been applied in the last century, that they give better results than a superior method could then give. But it still remains probable that the method depending on the observation of Venus in transit *is* superior to other modes of determining the sun's distance; and that when this method is applied with the improved instruments of our time its superiority will be rendered manifest.

PLATE VI.



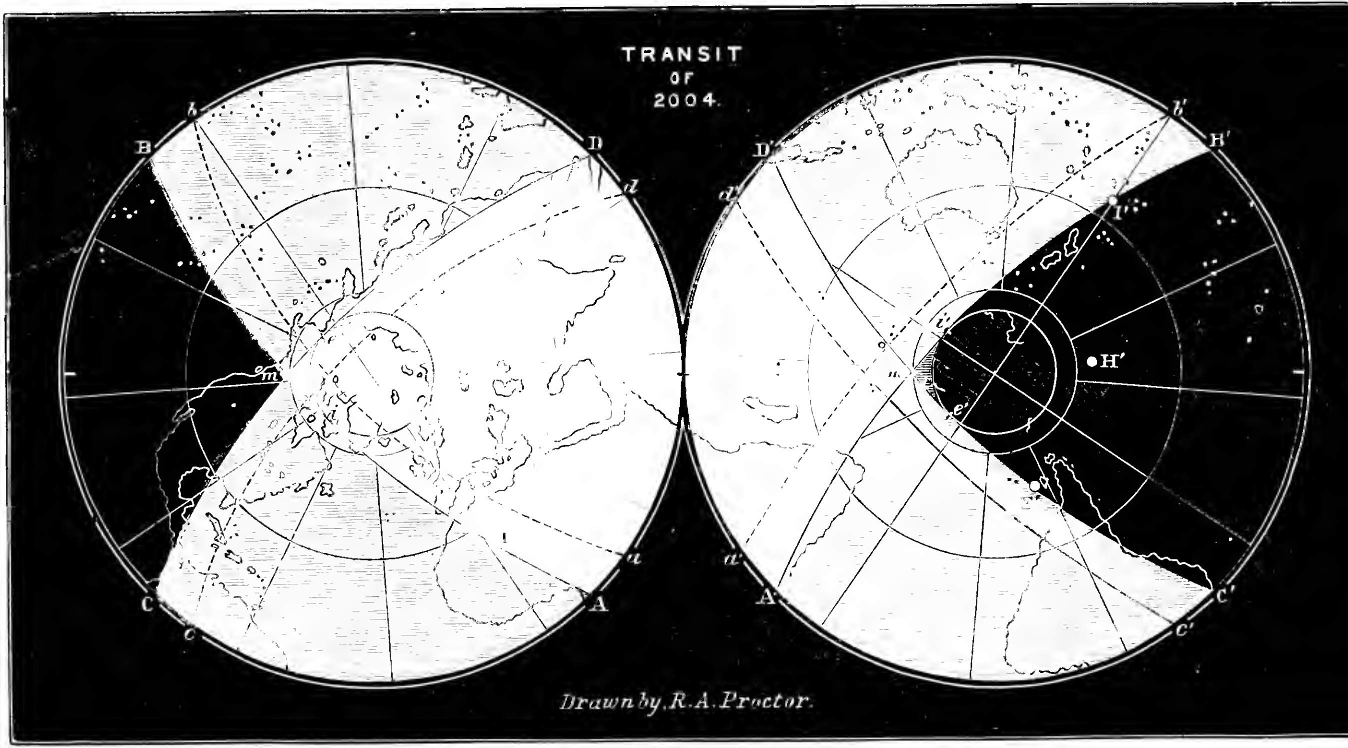
A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along *c d*, *c′ d′*, sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

PLATE VII.

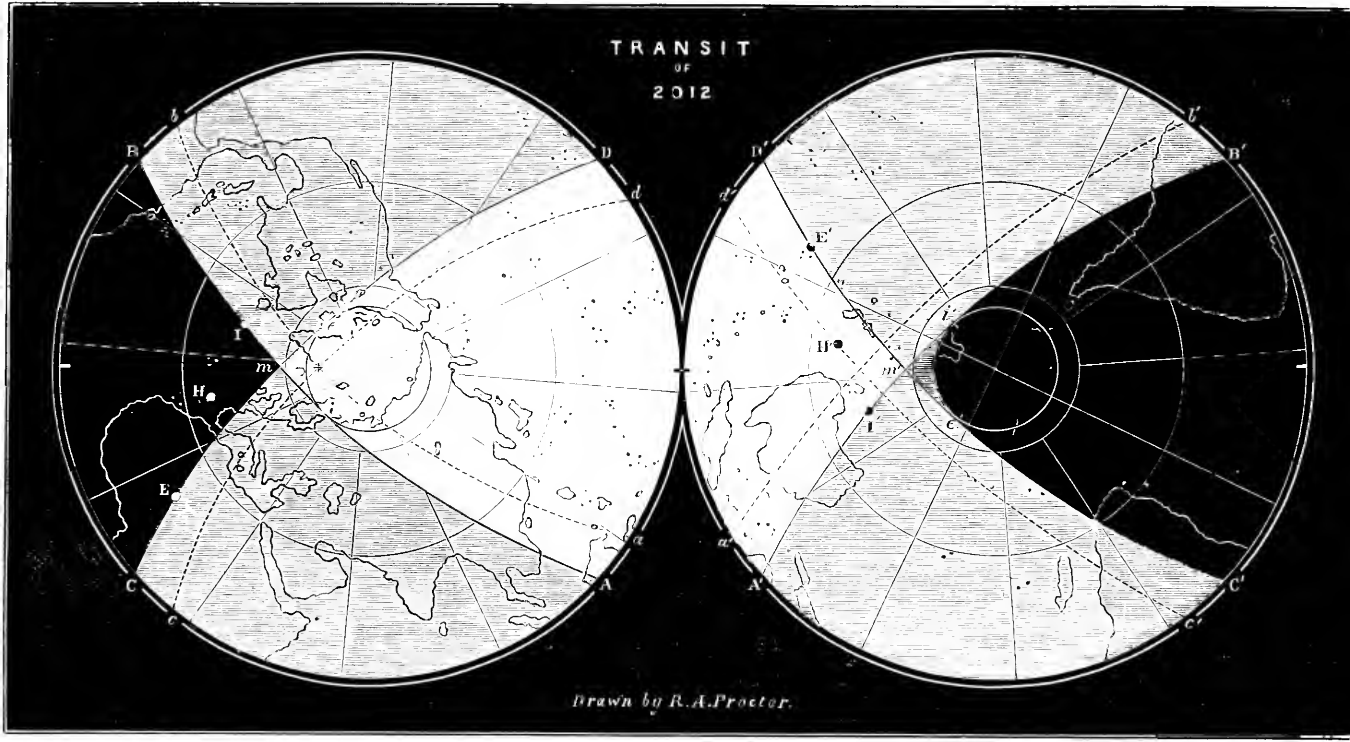


A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along *c d*, *c′ d′* sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

PLATE VIII.



A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along *c d*, *c′ d′*, sun 10° high at egress.



A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress: along *c d*. *c′ d′*, sun 10° high at egress.
I, I′, are the Delisleau poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

CHAPTER IV.

OF TRANSITS AND THEIR CONDITIONS.

Before we proceed to the consideration of the transits of 1874 and 1882, it will be desirable to enter on a more complete examination than heretofore of the general principles on which the determination of the sun's distance by observation of Venus in transit depends. To this subject the present chapter is therefore given. It deals with the various methods which are available for determining the sun's distance, the order in which transits recur, and lastly, the considerations on which the choice of stations will depend, in any given transit. These various points I wish to treat in an entirely popular manner, and therefore I shall leave out of account all those minor details which have to be considered in the complete discussion of the subject, referring the reader who may wish for a more thorough investigation of the matter to my 'Essays on Astronomy' and 'The Universe and the Coming Transits.'

First, then, let us consider the passage of Venus between the earth and the sun on the occasion of a transit, and see how the sun's distance may be inferred from the various appearances presented when the transit is viewed from different parts of the earth.

Let E E′ (fig. 13) be the earth, and V Venus passing between the earth and the sun (at S) on the course

FIG. 13.—Illustrating the general principles on which the determination of the Sun's distance by transit observation depends.

shown by the arrow, so that at the moment indicated by the figure a transit is in progress. At this moment let us suppose that from a northern station E′ Venus is seen projected upon the sun's face at *v′*, while from a southern station E she is projected at *v* (*v* and *v′* marking the place of her *centre*). It is to be noted that true perspective being quite out of the question, I here for convenience suppose the circle S to represent the *disc* of the sun seen from E, so that in considering what follows the reader need not trouble himself about the curved nature of the sun's *surface*.

Now, the *proportions* of the solar system being well known ever since the Copernican theory was established —or rather, since Kepler's laws were discovered—we know that the distance of V from the sun bears to the distance of E from the sun the proportion of about 72 to 100; whence, immediately, we see that E V bears to V *v* the proportion of about 28 to 72, or 7 to 18. And manifestly the opening-out of the lines V E and V E′ at the earth is less than their opening-out at the sun in this same proportion of 7 to 18 ; so that, for instance, if the two stations E and E′ are 7,000 miles apart (meaning the distance in a straight line, and for simplicity assuming that V E and V E′ are equal lines symmetrically placed with respect to the earth's globe), then $v\,v'$ is a distance of 18,000 miles. But such a determination as this, if justly and satisfactorily made, would in point of fact amount to a determination of the sun's size, and therefore of the sun's distance. Observe—the astronomer at E is supposed to have accurately determined the apparent position of Venus's centre at v, while the astronomer at E′ has accurately determined the apparent position of her centre at v' ; thus they know what proportion $v\,v'$ bears to the diameter of the disc S, that is, to the sun's diameter. Say, for instance, they find it to be the 47th part of this diameter. But they know also that $v\,v'$ is 18,000 miles in length. So that the sun's diameter is 47 times 18,000 miles, or 846,000 miles.

So soon, however, as we know the real size of the sun we know his distance. We know how large he looks, and a globe of given size can only present a certain apparent size at a certain distance. For example, a globe one inch in diameter looks just as large as the sun at a distance of about $107\frac{1}{3}$ inches, or a little less than 9 feet ;[1] a globe two inches in diameter

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[1] A halfpenny, which has a diameter of one inch, will be found to exactly conceal the sun when placed at a distance of $107\frac{1}{3}$ inches, the sun being at about his mean distance—that is, the observation being made in March, April, September, or October.

must be set twice as far away to look just as large as the sun; a globe three inches in diameter thrice as far away; and so on. In brief, the sun (like any one of these globes when placed as described) lies at a distance $107\frac{1}{3}$ times as great as his own diameter. So that multiplying 846,000 by $107\frac{1}{3}$ we get for the sun's distance (as resulting from the observations imagined above) 90,160,000 miles.

The considerations just discussed form the basis of all the various methods for determining the sun's distance by transit observations. These methods are only so many contrivances for bringing out the true result as satisfactorily as possible, by eliminating the various possible sources of error.

We may call the method just sketched the *direct method*, because it depends on the simple observation of the place of Venus on the sun's face. I shall have occasion presently to discuss the method somewhat more in detail. Let it suffice, here, to notice that the method presents manifest difficulties. The two observers, at E and E′, are of course not in direct communication; yet it is essential that their observations should either be made exactly at the same time or that at least the exact difference of time should be known. Again, it is not an easy matter to measure the place of Venus on the sun's face with the accuracy that the method requires. For these reasons Halley was led, in anticipation of the transit of 1761, to devise another method.

Let us suppose, for simplicity, that the two stations at E and E′ are not shifted by the earth's rotation while the transit lasts. In this case the observer at E would see Venus traverse such a path as *l v m*, while the observer at E′ would see her traverse the parallel path *l′ v′ m′*. The time occupied by Venus in each case would of course be proportional to the apparent length of the lines *l m* and *l′ m′*; so that if the time were accurately noted by the two observers, the apparent lengths of these lines would be known; whence, of course, the simplest possible geometrical considerations would give the position of the two chords and the apparent distance *v v′* separating them from each other. This known, the sun's size and distance follow as in the direct method. Seeing that the moment when Venus has just made her complete entry on the sun's face at ingress, and is just about to begin to leave his face at egress (in other words, the moments when her disc just touches the sun's edge on the inside), were supposed by Halley to be determinable with great accuracy, such a method as has just been described seemed to him admirably adapted for determining the sun's distance.

But clearly the difference of time in the imaginary case we have been dealing with, where the earth's rotation was neglected, will depend on the position of the chord of transit. Supposing Venus to traverse the centre of the sun's face, the two chords being equal in length, there would be no difference of time, while the difference would be great if the two chords were near the edge of the disc. In the latter case the method would be most advantageously applicable, while in the former it would not be applicable at all. Now, we have seen that the transit of 1761, as calculated by Halley (see pp. 34 and 35), was nearly central. Nevertheless, owing to the rotation of the earth, a difference of duration would occur in the case of such a transit. In a general way this has been already shown in the note on pp. 34 and 35. But it is also easy to show that a displacement comparable with the *v v′* of fig. 13 can be inferred from time observations applied as Halley suggested for the supposed conditions of the transit of 1761.

Fig. 14.—Illustrating the effect of the Earth's rotation on the motion of Venus in transit.

Let us suppose that in fig. 14 we are looking down upon the earth E and Venus V from the north, Venus travelling (with respect to the earth)[1] in the direction shown by the arrow. Let us suppose I E to represent a chord of transit across the face of the sun S. Now, the earth is rotating in the direction *w e* E along the arc *w e*; and as a transit may last several hours,

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[1] It is generally convenient to suppose the earth at rest, and Venus travelling only with the *excess of her motion* over the earth's motion around the sun.

a place which was at w when transit began (that is, when Venus appeared to be at I) would be carried by rotation to some point e by the time the transit ended (that is, when Venus appeared to be at E). In order to see the effect of such a rotation-shift on the apparent motion of Venus, let us take two lines, one from w, the other from e, through the centre of Venus (supposed at rest at V, near the middle of the transit) to the chord I E; we see that the line from the *earlier* position w passes to v, while the line from the *later* position e passes to v'. Thus the effect of the rotation of the earth during the time of transit, if considered alone, corresponds to a shifting forwards of Venus by the amount $v\,v'$. In other words, transit is shortened by the effect of rotation in direction $w\,e$. Suppose now another observer placed at the pole (whichever pole happened to be in sunlight at the time), so as not to be at all affected by rotation; or that, being placed near either pole, he were much less affected by rotation; or that, being placed on the side of the pole farthest from $e\,w$, the duration were *lengthened* through the effects of duration, instead of being shortened. Then there would arise on this account a difference of duration, which would lead to the determination of the sun's distance precisely as in the case before supposed. For in reality the result would be the determination of the apparent amount of the displacement $v\,v'$ along the chord of transit, corresponding to the known displacement $w\,e$ upon the earth; and the mere fact that both displacements are in an east-and-west direction does not render the observations less effective than those which in the former case gave the apparent displacement $v\,v'$ corresponding to the observers' displacement $\text{E}\,\text{E}'$, both displacements being on a north-and-south line.

In all ordinary cases, Halley's method depends partly on the distance of the observers measured in a north-and-south direction, and partly on the effect of rotation; and in the selection of stations both considerations have of course to be taken into account, the aim being to make the difference of duration as great, and therefore as exactly measurable, as possible. The considerations on which the selection of stations depends will be dealt with in a simple manner farther on.

We may conveniently call Halley's method the 'method of durations'—a name descriptive of the qualities of the method. But it certainly seems a mistake to limit the title 'Halley's method' to the case more particularly considered by him.[1] We may, therefore, use both names indifferently.

Halley's method requires the whole transit to be seen, or at least the beginning and end. Apart from other difficulties which this requirement introduces, the probability of favourable weather both at ingress or egress is manifestly less than the probability of

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[1] An effort has of late been made to dismiss from use the title 'Halley's method,' which Sir J. Herschel and others had long used. I cannot see why Halley's name should thus be summarily dismissed from the position it has so long occupied.

favourable weather for a single observation only. It occurred to Delisle, when preparations were being made for the transit of 1761, that assuming Halley was right in supposing the moment of contact at ingress could be determined with great exactness, a single observation of the sort might be employed instead of two terminal observations.

It is clear that the observer who sees Venus traverse such a chord as $l' m'$ (fig. 13) will see the transit begin earlier than one who sees her traverse such a chord as $l\,m$, for l is a point more *advanced* than the point l'. Suppose now that each observer notes the exact moment of local time when the transit begins (internal contact), and that, knowing his exact longitude, each can change his local time into Greenwich time; then these two Greenwich epochs will differ by an interval corresponding to the amount by which l is in advance of l'. But this gives a geometrical relation whence the distance between the chords $l\,m$ and $l' m'$ can manifestly be determined, just as well as though the length of each chord were ascertained. Hence $v\,v'$ becomes known, and thus, as in the direct method, the sun's size and distance can be determined.

Similar remarks apply (*mutatis mutandis*) to the observation of egress. The method, whether applied at ingress or at egress, is called Delisle's method.[1]

The employment of photography to record the

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[1] It is singular that Delisle's name, like Halley's, is not used in Sir G. Airy's programme for the transits of 1874 and 1882.

place of Venus on the sun's face at any particular instant need not detain us here, as it manifestly introduces no new astronomical relations.

And now let us consider how transits of Venus recur, in other words, how those opportunities are from time to time offered which admit of being utilised in the various ways above described.

Let us examine, first, how successive conjunctions of Venus are brought about.

Let the paths of Venus and the earth around the sun S (fig. 15) be represented by the circles V V′ and E E′; and let us suppose that Venus and the earth are, in the first instance, in conjunction, as at V, E, so that S V E is a straight line. We need take no account, at

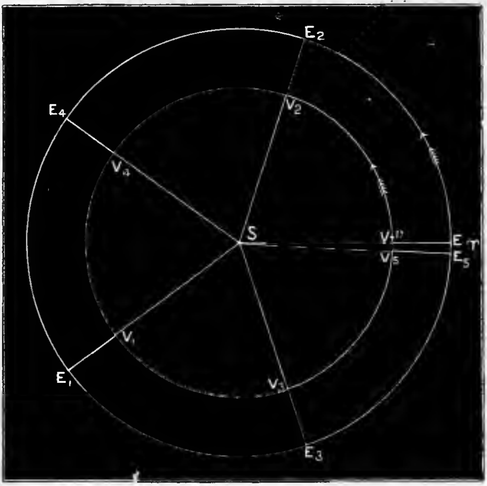

Fig. 15.—Illustrating the conjunctions of the Earth and Venus.

this stage, of the slight eccentricity of the two orbits, and of the fact that they are not exactly in the same plane. Thus, we may be supposed to be looking directly down upon the moving planets, which, instead of travelling, as they actually do, with a slightly varying velocity, are supposed to travel with their mean or average motion.

Now, the simplest way of determining when and where the two planets will be again in conjunction is perhaps the following:—

Imagine that a straight pointer from the sun to Venus, extending to the earth's orbit, like the line S V E, is carried round S as a central pivot by the motion of the planet Venus. Then whenever this pointer comes up to the earth, the three bodies—sun, earth, and Venus—are in conjunction. Now, Venus travels with a mean motion of 96′ 7″·8 per day around the sun (completing a revolution in 224·701 days), while the earth travels with a mean motion of 59′ 8″·3 (completing a revolution in 365·257 days [1]); so that in each mean solar day Venus gains, on the average, 36′ 59″·5 upon the earth. This is the rate at which our imaginary pointer, starting from a position such as S V E, sweeps onwards from the advancing earth, so as to again reach the earth by overtaking it, just as the minute-hand of a clock, after being in conjunction with the hour-hand, passes on towards its next conjunction, with the *excess* of its motion over the hour-

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[1] Sidereal revolution is here considered, not the tropical revolution which forms the year of seasons.

hand. We have only, then, to ask how long it will take the pointer, with its mean daily gain of 36′ 59″·5, to gain one complete circuit, to have the interval in time between successive conjunctions of the earth and Venus —in other words, there will be just as many days in this interval as the number of times that 36′ 59″·5 is contained in 360°, or, reducing both to seconds, as 2219·5 is contained in 1,296,000. The division is easily effected, and gives us 583·9 days.

Our Venus-carried pointer thus takes 583·9 days in overtaking the earth. This is more than a year by about 218·6 days, in which period, with her mean motion of 59′ 8″·3 per day, the earth travels round nearly 215½ degrees. Now, 216 degrees would be ⅗ths of a complete circuit. We see, then, that the next conjunction-line, S V′ E′, must be set almost exactly ⅗ths of the way round from S V E, or in the position $S V_1 E_1$; the next will have the position $S V_2 E_2$; the third will have the position $S V_3 E_3$; the fourth, the position $S V_4 E_4$; and the fifth will be close up to S V E, in the position $S V_5 E_5$, about 2½ degrees behind S V E.

Since the interval between each conjunction is about a year and three-fifths, the whole time occupied before the position $S V_5 E_5$ is reached by the conjunction-line will be five times 1⅗ years, or 8 years, less the short interval corresponding to the earth's motion over the arc $E_5 E$. We see, then, how it comes to pass that an interval of eight years brings round nearly the same circumstances as at the beginning of the interval, and why, therefore, when a transit has occurred, another may occur eight years later. A *second* interval of eight years, as we shall presently see, changes the conditions too largely (though they are still approximated to).

It may be mentioned in passing that since Venus gains one complete circuit on the earth between two successive conjunctions, and the earth goes nearly eight times round for the five conjunctions just considered, it follows that Venus goes nearly thirteen times round. In other words, thirteen revolutions of Venus are nearly equivalent to eight revolutions of the earth.

And now let us consider the effect of the inclination of the orbit of Venus to that of the earth, still, for the sake of simplicity, leaving out of account the slight eccentricity of the orbits.

If E E′, V V′ (fig. 16), represent the two orbits, and Æ be the place of the earth at the autumnal equinox, then the line E E′ represents the intersection of the two orbit-planes; and if, as before, we regard the plane of the paper as containing the orbit E E′, then the part V *v* V′ of the path of Venus is to be as regarded slightly above, the part V′ *v*′ V as slightly below, the plane of the paper. Accordingly, the end of the pointer which we have supposed Venus to carry round the sun, passes above the semicircle E *e* E′ and below the semicircle E′ *e*′ E. And supposing this pointer to be of the length S E, so that its end appreciably travels round E *e* E′ *e*′ (except for the displacement above and below the plane of this orbit), it is easy to calculate how much above or below the level E *e* E′ *e*′ the end of the pointer runs. When in the direction S E or S E′, of course the Venus-carried pointer has its extremity on the earth's path; when in direction S *v e* or S *v′ e′*, at right angles to E E′, the end of the pointer is at its farthest from the plane E *e* E′ *e′*. The inclination of the orbit of Venus being about 3° $23\frac{1}{2}'$, and the distance S *e* (the earth's distance from the sun) being about 91,430,000 miles, it is easily calculated that the extremity of the pointer passes above *e* and below *e′* at a distance of about 5,409,000 miles. At any other point, as P or P′, the end is above or below by an amount less than 5,409,000 miles in the same degree that P M or P′ M is less than *e* S or *e′* S (P M P′ being drawn square to E E′).

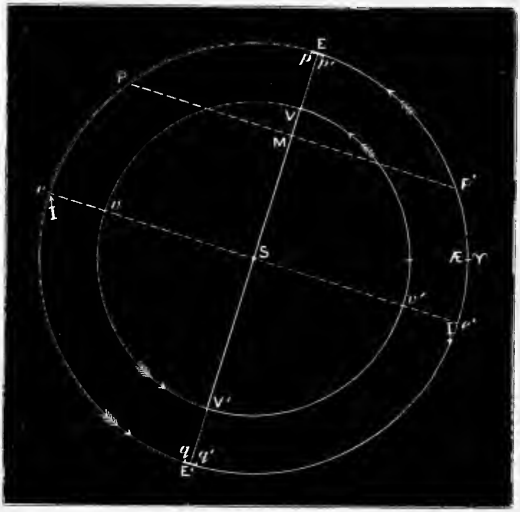

Fig. 16.—Showing the parts (*p p′* and *q q′*) of the Earth's orbit where transits can occur.

Now, it is clear that, for a transit to occur, a line from the sun's centre through Venus to the earth's orbit, at the time of a conjunction, must not pass more than a certain distance above or below the earth's orbit—that is, a conjunction must occur near the positions V E or V′ E′. And it is easy to determine roughly *how* near the earth must be to E or E′ at the time of conjunction, for a transit to occur. For let S V E, fig. 17, be our imaginary pointer at the time of a conjunction, and *s* V *e*, *s*′ V′ *e*′ lines touching the sun. Then it is manifest that if the earth be anywhere on the line *e*′ E *e* at the time of conjunction, a line from the earth to Venus must meet the globe *s* S *s*′, or, in other words, there is a transit. But if the earth be above *e*′ or below *e* at the moment of conjunction, there can be no transit. Now, S *s*, the sun's radius, is about 426,000 miles, and therefore E *e* and E′ *e*′ are each less than 426,000 miles in the proportion in which V E is less than V S, or, roughly, as 277 to 723; so that E *e* and E *e*′ are each equal to about 163,000 miles—a small distance compared with the actual range of the end of our Venus-carried pointer above and below the earth's orbit. And it is easily calcu-

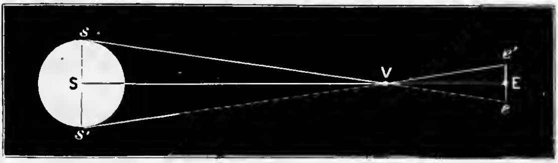

Fig. 17.—Illustrating the occurrence of transits.

lated[1] that the range on either side of E or E′ (fig. 16), within which a transit is possible, is represented by the arcs p E p' and q E q', each equal to about 3½ degrees.

Now, having found that the circuit of the earth's orbit has these two *transit-regions,* so to call them, it is not difficult to ascertain the general conditions under which the conjunction-line will fall from time to time upon one or other region.

Let it first be noted that the points E and E′ are at present those traversed by the earth on or about December 7 and June 6. The line E S E′ does not, however, bear a fixed position with respect to the point Æ, but the points E and E′ slowly shift forwards, that is, in the direction indicated by the arrow. The node of Venus's orbit shifts backwards with respect to the stellar sphere by about 20″·5 per annum; but as the point Æ shifts backwards annually by about 50″·1 (the precession of the equinoxes), it follows that the nodes V and V′, and therefore the points E and E′, advance with respect to Æ by about 29″·6 (the excess of 50″·1 over 20″·5) annually. Still, in dealing with the general question of the recurrence of transits, we must not regard the node of Venus as advancing by 29″·6 annually, but as receding by 20″·5; for in what

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[1] We require to have

$$\frac{\mathrm{E}\,p}{e\,\mathrm{S}}=\frac{163{,}000}{5{,}409{,}000}$$

that is, the sine of the arc $\mathrm{E}p = 163 \div 5409$. Whence $\mathrm{E}p$ is an arc of about 1° 44′, and each of the arcs pp' and qq' about 3° 28′.

has hitherto been said about successive conjunctions of Venus and the earth, we have used the sidereal periods of both planets, and we cannot substitute the tropical year without making corresponding corrections.

We may regard the system of five conjunction-lines shown in fig. 15 as a spoked wheel, which slowly but continuously shifts backwards in such sort that any one spoke, S E, shifts to the position $S_5 E_5$ in eight sidereal years *less* the time occupied by the earth in moving over E_5E, or about 2·449 days. This shift of position amounts to rather less than 2° 25′; but as the transit regions are themselves shifting backwards at the rate of 20″·5 annually, or about $2\frac{3}{4}'$ in eight years, we have the shift of the conjunction-lines, with reference to the transit regions, equal to about 2° 22′ in eight years.

Now let us suppose that the conjunction-line has at starting the position which it actually had on the occasion of the transit of the year 1631. Thus, let $p\,p'$ (fig. 18) represent what may be called the *December transit region*, and $q\,q'$ the *June transit region*, and let S V E, the first conjunction-line, fall so that E is the place of the earth on December 6.[1] The five next conjunction-lines have, as already shown, the positions $V_1 E_1$, $V_2 E_2$, $V_3 E_3$, $V_4 E_4$, and $V_5 E_5$; and we see that E_5 being 2° 22′ from E, while $p\,p'$ is an arc of nearly $3\frac{1}{2}°$, E_5 falls within $p\,p'$, and there is again a transit, on or

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[1] In the seventeenth century, but corresponding to her position on December 9 in the nineteenth century.

about December 4.[1] This corresponds to the transit of 1639. The next five conjunctions take place in due

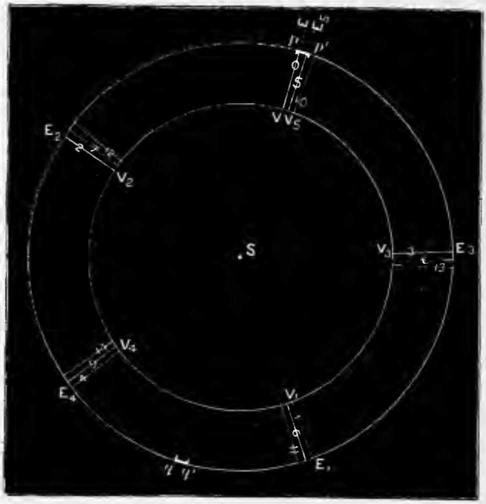

Fig. 18.- Illustrating the regression of the conjunction-lines over a transit region ($p\,p'$).

order on the lines marked 6, 7, 8, 9, &c. We see, then, that there will be no December transits, that is, no conjunction within the arc $p\,p'$, until the gradual advance of the conjunction-line $E_2\,V_2$ has carried it by eight yearly steps to the transit region $p\,p'$. This manifestly requires as many eight-yearly intervals as 2° 22′ is contained in the arc $E_1\,E_2$, or roughly the fifth part of the complete circuit; or, in other words, we must multiply $30\frac{3}{8}$ by 8 to obtain roughly the number

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[1] In the seventeenth century, or about December 6 in the nineteenth.

of years. This gives 243 as the nearest whole number of years; and this, it will be noted, is the interval from the December transit of 1631 to the next December transit of 1874, or from the June transit of 1761 to the next June transit of 2004. But we see that while the conjunction-line $E_2 V_2$ is travelling by eight yearly steps to the transit region $p\,p'$, the conjunction-line $E_1 V_1$ will have travelled by similar steps to the position $E_4 V_4$, passing over the transit region $q\,q'$, and giving therefore two June transits in the middle of the period of 243 years.

And here, for the first time we have to note the effects of the slight eccentricity of the orbits of the earth and Venus. If the two paths were concentric circles their centre being the sun, the conjunction-lines would be distributed with perfect uniformity, so that the arcs $E\,E_2$, $E_2\,E_4$, $E_4\,E_1$, $E_1\,E_3$, and $E_3\,E_5$ would be exactly equal; but owing to the eccentricity of the orbits, and the consequent variation in the motions of both Venus and the earth, this uniformity does not hold. The five arcs just named, or others similarly formed from any other conjunction as a starting-point, are slightly different in length, being largest always when the earth's orbit approaches nearest to that of Venus, so that the angular motions of the two bodies around the sun differ least, and smallest where the orbits are farthest apart so that the angular motions of the two bodies differ most.[1]

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[1] Anything like an exact discussion of the varying relative motions

At the present time, for instance, the conjunction-lines have such positions as are indicated in fig. 19,

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of Venus and the earth would be altogether out of place in a work of this nature. Let it suffice here to note the following values :—

	The Earth			Venus		
	°	′	″	°	′	″
Maximum daily motion . .	1	1	10	1	37	30
Mean ,, ,, . .	0	59	9	1	36	8
Minimum ,, ,, . .	0	57	11	1	34	52

Now, the perihelion of Venus is in longitude about 124¼°, the perihelion of the earth in longitude about 99½°, or nearly 25° behind. As the eccentricity of the earth's orbit is greatest, being nearly twice that of Venus's orbit (if measured in miles, still greater), we should not be far wrong in taking the earth's perihelion for the point of nearest approach to the orbit of Venus; but inasmuch as opposite this point Venus is approaching perihelion, we somewhat diminish the longitude to obtain the actual point of nearest approach, which will be in about 70° of longitude, or at the place occupied by the earth on or about December 2. Here the daily motion of the earth is about 1° 0′ 56″, that of Venus about 1° 36′ 49″, the excess of the motion of Venus in longitude being therefore 35′ 44″. [When the earth is in longitude 90° her mean daily motion is about 1° 1′ 7·5″, that of Venus in the same longitude being about 1° 37′ 0·5″, an excess of 35′ 53″; so that the daily motions are not so nearly equal as in longitude 70°. In fact, it chances that the motions of Venus and the earth in conjunction are nearest to equality almost at the time corresponding to a December transit.] Now, at the opposite part of the two orbits, or in longitude about 250°, we have the earth's daily motion about 57′ 29″, that of Venus 1° 35′ 19″, an excess of about 37′ 50″, or more by about 2′ 6″ than that in longitude 70°. It follows necessarily that successive conjunction-lines (after successive eight-yearly periods) fall nearer together in the June part of the orbits than in the December part. For the exact eight years which carry the earth from position E (fig. 15), to position E again soon after conjunction at E_5 with Venus at V_5, correspond to thirteen complete revolutions of Venus plus 0·955 days, wherever E may be. Now let v be the place reached by Venus when the earth is at E, then V v is the space traversed by Venus in 0·955 days. But V v also measures the gain of Venus on the earth, while the earth has been passing from E_5 to E. Now, in longitude 70°, Venus, being nearer her perihelion, moves faster than in longitude 250°; hence on this account the arc V v will be greater for a December conjunction than for a June one. Since, then, the gain V v of Venus is greater at a where the eccentricities of the two orbits are properly shown, and the conjunction-lines are placed in longitude

December conjunction than at a June one, while yet it accrues at a less rate as we have seen above, it follows that it requires a longer time to accrue: in other words, the arc corresponding to $\mathrm{E}_5\,\mathrm{E}$ requires a longer time, and if the earth moved uniformly would be a longer arc at a December conjunction than at a June one. But the earth is moving faster in December than in June; *à fortiori* therefore the arc corresponding to $\mathrm{E}\,\mathrm{E}_5$ will be greater for a December than for a June conjunction. Thus is explained the greater distance between the transit lines of a December pair than between the corresponding lines of a June pair. See Plate I. To further illustrate this, and also to make this reasoning more directly applicable to the subject-matter of this chapter, I will now proceed to calculate the actual displacement of the conjunction-line in eight years, for the two transit regions respectively.

Suppose a conjunction to occur on or about $\left\{\begin{matrix}\text{December 9}\\ \text{June 6}\end{matrix}\right\}$. Then in eight sidereal years from this conjunction the earth has gone eight times round, while Venus has gone round thirteen times *plus* her motion in 0·955 *d*. This motion takes place at the daily rate of $\left\{\begin{matrix}1^\circ\,36'\,45''\\ 1^\circ\,35'\,13''\end{matrix}\right\}$, and therefore places Venus in advance of the earth by $\left\{\begin{matrix}5544''\\ 5456''\end{matrix}\right\}$; and the daily gain of Venus, or $\left\{\begin{matrix}2147''\\ 2269''\end{matrix}\right\}$ is contained $\left\{\begin{matrix}2{\cdot}582\\ 2{\cdot}405\end{matrix}\right\}$ times in $\left\{\begin{matrix}5544''\\ 5456''\end{matrix}\right\}$. Therefore conjunction must have occurred $\left\{\begin{matrix}2{\cdot}582\,d\\ 2{\cdot}405\,d\end{matrix}\right\}$ earlier, or since the earth's daily motion is $\left\{\begin{matrix}3650''\\ 3444''\end{matrix}\right\}$ conjunction must have occurred $\left\{\begin{matrix}2^\circ\,37'\,26''\\ 2^\circ\,18'\,\ \ 1''\end{matrix}\right\}$ in longitude behind the conjunction-line of the earlier transit. Diminishing each arc by 2¾′ for the change of the nodal line in eight years, we obtain a motion (with respect to the node) of about $\left\{\begin{matrix}2^\circ\,35'\\ 2^\circ\,15'\end{matrix}\right\}$,—near enough for our present purpose.

In the above, no account is taken of perturbations of the motions of Venus and the earth by the other planets.

It will be convenient to add here a more exact calculation of the transit arcs $p\,p'$ and $q\,q'$, fig. 16. We may follow the same plan as at page 107.

3°, 77°, 155°, 225°, and 292°, which correspond, nearly enough for our purpose, to the inferior conjunctions of

We have,—the distance of Venus from sun at $\left\{\begin{matrix}\text{ascending}\\ \text{descending}\end{matrix}\right\}$ node is $\left\{\begin{matrix}65.865{,}000\\ 66{,}394.000\end{matrix}\right\}$ miles (where the earth's mean distance is taken as 91,430,000 miles), and the earth's distance in the same longitudes respectively is $\left\{\begin{matrix}90{,}036{,}000\\ 92{,}817{,}000\end{matrix}\right\}$ miles; so that the distance of the earth from Venus at conjunction is respectively $\left\{\begin{matrix}24{,}171.000\\ 26{,}423{,}000\end{matrix}\right\}$ miles; and diminishing the sun's radius (which here for greater exactitude we take at 426,450, its true value if sun's mean distance be 91,430,000 miles) in the ratio $\left\{\begin{matrix}24{,}171{,}000 : 65.865{,}000\\ 26{,}423{,}000 : 66{,}394{,}000\end{matrix}\right\}$, we obtain for the distance corresponding to $\text{E}\,e$ and $\text{E}\,e'$ fig. 17 the value $\left\{\begin{matrix}156{,}500\\ 169{,}720\end{matrix}\right\}$ miles; and it thence follows that

$$\begin{aligned} p\,\text{E} &= \text{E}\,p' = 156{,}500 \operatorname{cosec}(3^\circ\ 23\tfrac{1}{2}') = 2{,}645{,}300 \text{ miles}\\ q\,\text{E}' &= \text{E}'\,q' = 169.720 \operatorname{cosec}(3^\circ\ 23\tfrac{1}{2}') = 2{,}868{,}700 \text{ miles}\end{aligned}$$

while the arc-measure of $p\,\text{E}$, or $\frac{p\,\text{E}}{\text{E S}}$, is equal to 1° 41′, so that $p\,p'$ is an arc of 3° 22′;

and the arc-measure of $q\,\text{E}'$, or $\frac{q\,\text{E}'}{\text{E}'\,\text{S}}$, is equal to 1° 46′, so that $q\,q'$ is an arc of 3° 32′.

These values are for the centres of Venus and the earth. It would be easy, but is scarcely worth while, to calculate them for exterior or interior contact, and for the whole earth,—that is, to determine the arc $p\,p'$ or $q\,q'$ for the extreme cases where if any part of Venus be seen on the sun's disc from any part of the earth, a transit shall be considered to have taken place, or where no transit shall be considered to have taken place unless the whole of Venus be seen within the sun's disc even from the station which throws her farthest from the sun's centre at the moment of nearest approach. Into such niceties, however, we need not here enter, as they are merely questions of curiosity, and neither present any difficulty nor involve any important principle.

It will be seen that since at two successive conjunctions near December 7, the conjunction-lines are separated by 2° 37′ 26″ (or about 2° 35′ measuring from the node), while the transit arc is about 3° 22′ in range, whereas at two successive conjunctions near June 5, the conjunction-lines are separated by only 2° 18′ 1″ (or about 2° 15′ measuring

Venus on the dates, September 26, 1871, December 9, 1874, February 24, 1870, May 5, 1873, and July 14, 1876. Now it is clear that the conjunction-line $V_5\ E_5$ is farther from the nodal line $V'\ E'$ than is the conjunction-line $V_3\ E_3$. In fact the longitude of V', the node, is about $255\frac{1}{3}°$; and V_3 is only $30\frac{1}{2}°$ or so from the node, while V_5 is about $36\frac{1}{2}°$ from the node. Hence the conjunction-line $V_5\ E_5$ will take longer, in marching up by eight yearly steps to V', than the half of the period of 243 years, which is the time in which it comes up to the

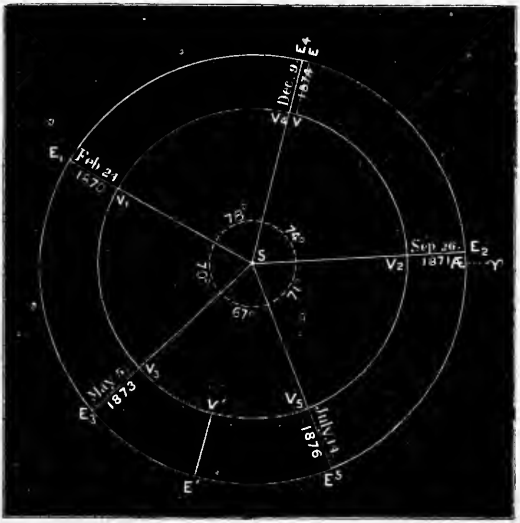

Fig. 19.—Showing the actual position of conjunction-lines of the earth and Venus.

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from the node), and the transit arc has a range of 3° 32′, there is a much greater chance of a pair of transits when the conjunction-line is sweeping over the June transit-region, than when it is sweeping over the December transit-region.

position V_3 E_3. We need not enter here into the calculations by which the exact time occupied in each part of the progression is determined. Let it suffice that the considerations just adduced serve to explain how it is that from June 1761 the epoch of the first transit of the last pair, to December 1874 the epoch of the first transit of this century's pair, is a period of only 113½ years, whereas from December 1874 to June 2004 the epoch of the first transit of the next pair, is a period of 129½ years. The former is the time occupied by the conjunction-line in moving from the node V' to the position V_3, when of course the other conjunction-lines have the position shown in fig. 19, and December transits occur; while the latter is the longer time occupied by the conjunction-line V_5 E_5 in moving up to V' E'. The sum of the two periods 113½ and 129½ is the period 243, just mentioned as that which separates two first transits either of a June pair or of a December pair. It is clear that if we start from the first of a June pair, we have the following intervals between successive transits: 8 years, 105½ years (8 from 113½ years), 8 years, 121½ years (8 from 129½ years), and so on continually in the order 8, 105½, 8, 121½, so long as there is no break on account of a single transit occurring instead of a pair. This can manifestly happen, both at the December region and at the June region, though more readily at the former than at the latter. The conjunction-line steps back (so to speak) over the December region by steps of about 2° 35′ (see note, p. 113), and this region is about 3° 22′

in width; so that if the first step falls on the beginning of the interval or within 45′ of it, the next will fall within the transit region, and there will be two transits · but if the first falls anywhere between 45′ and 2° 35′ of the beginning of the transit region the next will fall outside. The number of occurrences of a pair of transits in the December region will therefore bear to the number of occurrences of but one transit, the proportion which 45′ bears to 1° 50′, or which 9 bears to 22. That is, on the average of a great number, there will oftener be one transit only in the December region than a pair. Applying the same reasoning to the June period, we have (see note, p. 113) 2° 15′ for the regression and 3° 32′ for the width of the transit-region, giving 77′ favourable for the occurrence of a pair of transits, and 58′ for the occurrence of but one. Hence, on the average of a great number of cases of June transits there will be a pair oftener than a single transit, in about the proportion of 77 to 58, or nearly as 4 to 3.

It chances, however, that the interval between one December set and the next, or between one June set and the next, so nearly reproduces the same exact circumstances, that when a pair of transits has occurred in one instance it is almost certain that on the next occasion there will be a pair also. Accordingly, for many successive passages of the December transit-region, and for a yet greater number of successive passages of the June transit-region, there will be a pair of transits at each passage. Then will follow long intervals during which each passage will bring but a single transit. The series 8, 105½, 8, 121½, 8, &c. will then be modified into the series 113½, 129½, 113½, &c. But various other modifications occur in the course of long periods of time. Thus the triplet of intervals 105½, 8, 121½, in the complete series may be changed either into the pair 113½, 121½, or into the pair 105½, 129½; while the triplet 121½, 8, 105½, may be changed either into the pair 129½, 105½, or into 121½, 113½, according to circumstances.

So much for the order in which transits recur, either at the ascending node in December or at the descending node in June. Let us now consider how stations are selected for applying the various methods which are available.

Let S, fig. 20, represent the sun, and V Venus, the arrows showing the direction in which Venus

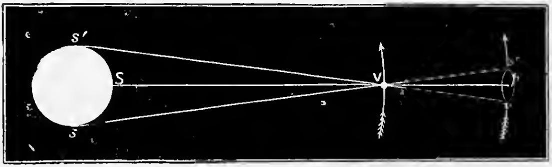

Fig. 20.—Illustrating Venus's shadow-cone.

and the earth are travelling around S. Let S V *p* represent the Venus-carried pointer of which we have already made frequent use, its extremity *p* being in the figure rather above the earth's orbit, and travelling onwards with the excess of Venus's motion, so as to overtake the earth. Now let a cone, having the centre

PLATE X.

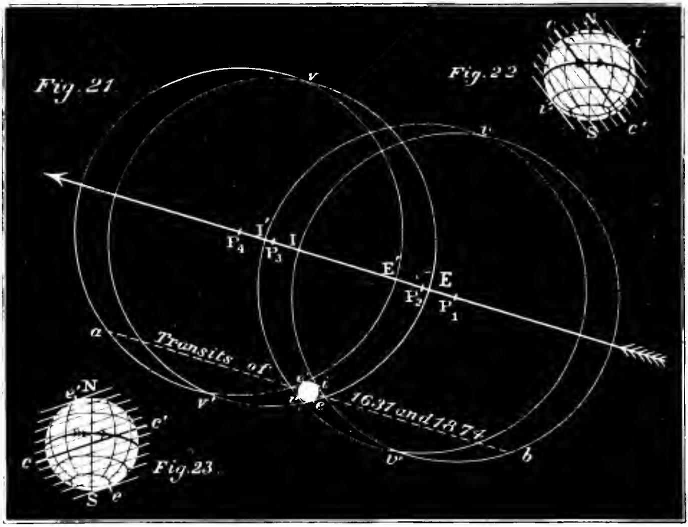

Fig. 22, Ingress 1631 and 1874. Fig. 23, Egress 1631 and 1874.

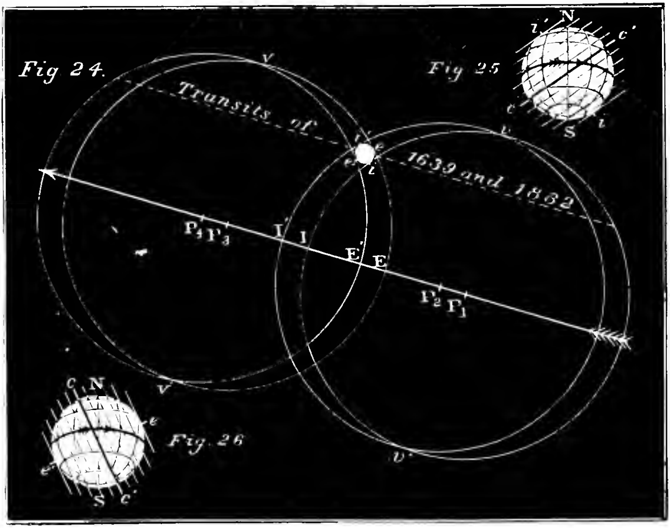

Fig. 25, Ingress 1639 and 1882. Fig. 26, Egress 1639 and 1882.

ILLUSTRATING PASSAGE OF VENUS'S SHADOW-CONE OVER EARTH IN 1631, 1639, 1874, AND 1882.

of Venus, V, for its vertex, and S V for its axis, be supposed to envelope the sun after the manner shown by the section *s* V *s′* in the figure, and let the prolongation of this cone beyond V, be *v* V *v′*, *v v′* being a circular section through *p*. Then we may regard this circular section (which corresponds to *e e′* in fig. 17) as travelling onwards like a gigantic wheel more than 300,000 miles in diameter, to overtake E; and if V is near enough to a node, then will this great circle pass athwart E in such sort that E will traverse a chord of the circle *v v′*. Let us try to picture such a passage. Suppose V to be near an ascending node so that the circle *v v′* as it overtakes E has a slight upward motion: also if we are looking from S towards E (and *v v′* were a real circular outline) we should see *v v′* moving from right to left to overtake E. It will be convenient to regard E as at rest so that we consider only the *excess* of the motion of *v v′* over that of the advancing earth.

In fig. 21, Plate X., *v* I *v′* represent the circle *v v′* of fig. 20 on an enlarged scale at the moment when the earth *e e′* is first touched at the point *i*. At this moment an observer at *i* will of course see the centre of Venus just crossing the edge of the sun. (This is manifest from fig. 20, where we see that a line drawn to V from any point, as *i*, fig. 21, on the surface of the cone *v* V *v′* will touch the globe *s s′*.) To an observer at *i* then, but to no one else on the earth *e e′*, transit will have begun (reference being always made to the centre of Venus).

The centre of *v v′* advancing from P_1 to P_2, the circular section reaches the position *v* I′ *v′* touching the earth at *i′*. Its edge has all this time been passing over the face of the earth *e e′*, moving nearly parallel to itself; and transit has now begun for every part of the illuminated hemisphere of the earth. So that *i′* is the place on the earth where transit begins latest. We have then *i* the place of earliest beginning (or, as it is technically called, *the pole of accelerated ingress*), and *i′* the place of latest beginning (or the *pole of retarded ingress*). The points *i* and *i′* are not *exactly* opposite each other even on the *circle e e′*,[1] still less are they exactly opposite points on the *globe e e′*, which has been rotating all the time that the centre of *v v′* has occupied in advancing from P_1 to P_2, a process lasting several minutes (more than 25 m. for instance in the transit of 1874, and 17 m. in the transit of 1882).

Still, as a first approximation, we may consider the points *i* and *i′* to be at opposite extremities of a diameter of the earth, taking (in order to reduce errors as much as possible), the face of the earth turned sunwards when the advancing edge of *v v′* crosses the centre of the earth's disc, and taking that diameter *i i′* of the earth which is at right angles to the advancing edge in this intermediate position. With this assumption, the passage of the edge of the circle *v v′* over the

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[1] For a tangent to *v* I *v′* at *i* is not parallel to a tangent to *v* I′ *v′* at *i′*, whereas tangents at the extremity of any diameter of a circle are necessarily parallel. (In fig. 22, the tangents at *i* and *i′*, are taken parallel to a tangent to the circle *v v′* at the point where, and at the time when, its edge crosses the centre of the disc *i i′* of fig. 21.)

earth's face is illustrated by fig. 22 (Plate X.) which represents on an enlarged scale the disc *i i′* of fig. 21, the edge of the circular shadow being represented in ten successive stages of its (supposed) uniform advance. The earth is shown in the proper position for a December transit. Nothing can be easier than to determine the position of *i* and *i′*, the poles of accelerated and retarded ingress. For all the circumstances of the motion of Venus are known, whence the motion of the projection of her centre along P_1 P_2 P_3 P_4 is determined. With reference to the earth *e e′* we know also what face of the earth is turned sunwards at the moment when the section *v v′* crosses the centre of the earth's disc. The size of the section *v v′* is also known (see note at p. 114 where it is calculated both for December and June transits), except of course in so far as it depends on the more exact determination of the sun's distance. And indeed, we see from this in a new way, how the circumstances of the transit as viewed at different stations depend on the distance of the sun, and therefore conversely how our estimate of the sun's distance depends on the circumstances of the transit as viewed at different stations. For while the disc *e e′* of the earth has a known diameter of about 7,900 miles, and the section *v v′* has the position and path of its centre along P_1 P_4 determinable independently of the sun's distance, the size of the section (as we see from the note, p. 114) depends on the sun's distance and size. Now if we enlarge or diminish *v v′*, while leaving *e e′* unchanged in position, and the motion of the centre of $v\,v'$ along $P_1\,P_4$ also unchanged, we manifestly modify the nature of the passage of the edge of $v\,v'$ over the disc $e\,e'$.

The section $v\,v'$ passing on, arrives at length at the position $V\,e\,V'$ touching the disc of the earth at e. At this moment the centre of Venus is seen, by the observer at e, on the edge of the sun; in other words, egress (of the centre of Venus) is taking place, and e is the station where egress is first seen. The section $v\,v'$ passes on until it has the position $V\,e'\,V'$, when it is about to leave the earth finally, its last contact with the earth being at e'—where egress takes place latest. In the interval the edge of $V\,V'$ has been passing over the disc $e\,e'$, moving nearly parallel to itself: we have then e the *pole of accelerated egress* and e' the *pole of retarded egress.* As in the case of ingress, e is not exactly opposite to e' even on the *circle* $e\,e'$, while rotation has affected the *globe* $e\,e'$, so that e is still farther from being opposite to e' on the earth.

We may, however, in this as in the former case, regard (for a first approximation) e and e' as points on opposite extremities of a diameter of the earth, taking the moment intermediate between earliest and latest egress. With this assumption, the passage of the edge of the circle $V\,V'$ across the earth's face is illustrated by fig. 25 (Plate X.), which represents the disc $e\,e'$ of fig. 21 on an enlarged scale, the edge of the circular shadow being shown in ten successive stages of its supposed uniform retreat. The earth is shown in the proper position for a December transit.

It need hardly be said that the face of the earth turned sunwards when the section $v\ v'$ has advanced to the position $V\ V'$ is greatly changed from the face which had been turned sunwards when ingress was in progress. But the time of egress is easily calculable, like that of ingress, from the known motions of Venus and the earth; the face of the earth turned sunwards is also known; and all the circumstances of the passage of the edge of $V\ V'$ over the earth's face at egress are easily determined. In fact, all that was said respecting ingress is true, *mutatis mutandis,* in the case of egress.

The conditions represented in fig. 21 are actually those of the transit of 1874. The shadow-cone of Venus passes slantingly upwards with reference to the earth, and the centre of the circular section $v\ v'$ passes north of the earth. The earth passes, therefore, through the shadow section as along the dotted line, in the manner shown farther on in fig. 35. But the conditions of the transit of 1631 so nearly resembled those of the coming transit that fig. 21 conveniently illustrates both transits.

In order to more thoroughly master the above reasoning, the reader would do well to run over it again, using figs. 24, 25, and 26, instead of figs. 21, 22, and 23 respectively. Fig. 24, with its companion projections, illustrates the transit of 1882 (and approximately also the transit of 1639). See also fig. 35.

Then the reader can apply the explanation given above, with very slight changes, to the case of June transits, illustrated by fig. 27. Figs. 28 and 29 are the companion June projections of the earth for the earlier of a pair of June transits (as the transits of 1761 and 2004), while figs. 30 and 31 are the companion projections for the later of a pair of June transits (as the transits of 1769 and 2012). Moreover it chances, so nearly similar in position are the northern and southern transit chords for the four transits just

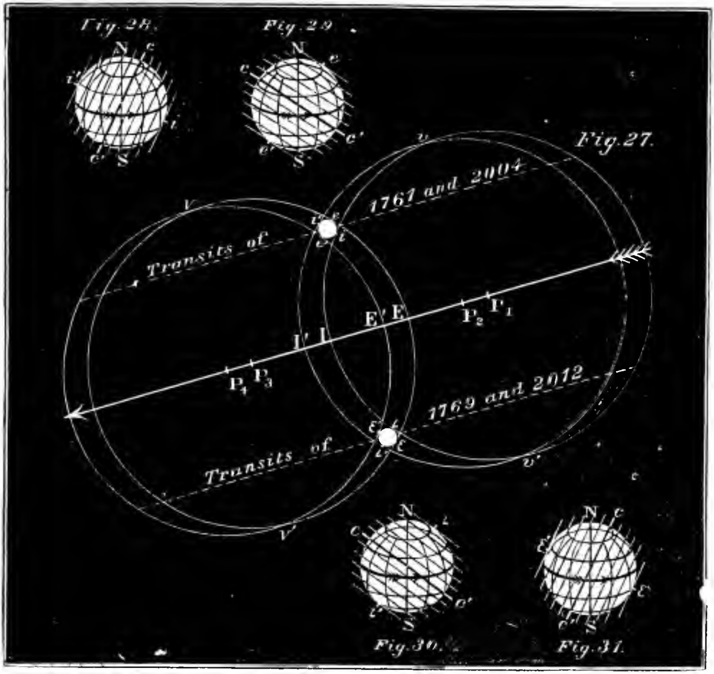


Fig. 27.—Illustrating the passage of Venus's shadow-cone over the earth during the transits of 1761, 1769, 2004, and 2012.

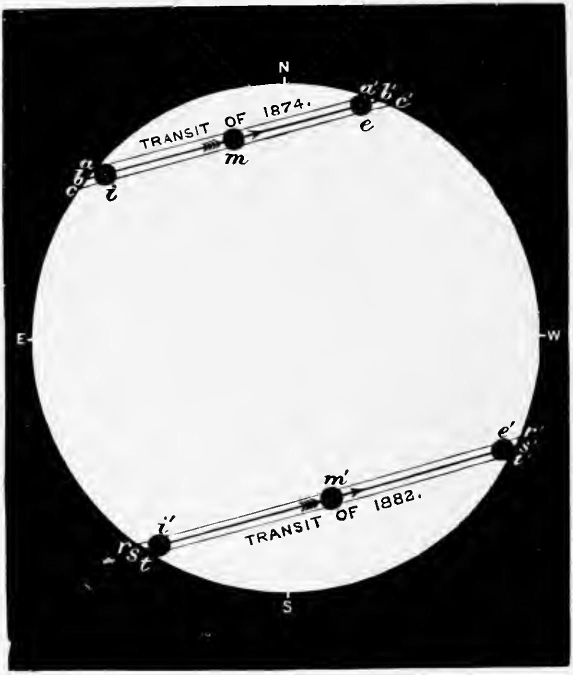
Fig. 28.—Ingress, 1761 and 2004.

Fig. 29.—Egress, 1761 and 2004.

Fig. 30.—Ingress, 1769 and 2012.

Fig. 31.—Egress, 1769 and 2012.

PLATE XI.



TRANSITS OF 1874 AND 1882.

ILLUSTRATING INTERNAL CONTACTS AND MID-TRANSIT, AND SHOWING RELATIVE DIMENSIONS OF THE DISCS OF VENUS AND THE SUN.

named, that one and the same figure illustrates all four with sufficient approximation for illustrative purposes.

Now it is easy to see how Delislean stations are to be selected in any given case. Take the transit of 1874. We find first the hours at which the circle *v v′* (fig. 21) crosses the centre of the disc of the earth *i e i′ e′* at the beginning and end of the transit, or, which is precisely the same thing, the moment when the centre of Venus, as seen from the earth's centre, reaches the positions *b* and *b′*, Plate XI. This is in point of fact the 'calculation of the transit,' and depends on principles corresponding to those involved in the calculation of an eclipse. Having these two epochs of the beginning and end of transit, and also the positions of the transit chord *b b′* Plate XI., and *a b* of fig. 21 Plate X., we make a sun-view of the earth at the beginning of the transit as Plate XII., and another of the earth at the end of the transit as Plate XIII. (Plates XII. and XIII. really represent the aspect of the earth for the times of internal contact—illustrated in Plate XI.—at the beginning and end of transit, but they sufficiently illustrate the present description).

Then the positions of the points *i* and *i′* fig. 21, Plate X. are known at once, from the geometrical relations pictured in fig. 21,[1] and thus we have the poles of accelerated and retarded ingress placed as *i* and *i′* in fig. 22, Plate X., or as A and B in Plate

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[1] Of course the vertical line N S in figs. 21, 24, and 27, represents north and south line of the earth's disc *i e i′ e′*.

XII., these positions being the same, it will be observed, as the positions of *i* and *i′* on the small disc *i e i′ e′* of fig. 21. The observers of the accelerated ingress must be near *i* on the illuminated hemisphere *i i′*, fig. 22, while the observers of retarded ingress must be near *i′*. The amount of acceleration or retardation will depend on the distance from *c c′*, a station at any point in any one of the parallels in fig. 22 having equal acceleration or retardation (according as the parallel is nearer *i* or *i′*). The parallel lines of the figure are of course circles on the globe of the earth; and *i* and *i′* are the poles of these circles.[1]

Again, the positions of the points *e* and *e′*, fig. 21, Plate X., are known; and thus we have the poles of accelerated and retarded egress placed as *e* and *e′* in fig. 23, Plate X., or as C and D in Plate XIII., these positions being the same as those of *e* and *e′* in the small disc *i e i′ e′* of fig. 21. The observers of the accelerated egress must be placed near *e* on the illuminated hemisphere *e e′* fig. 23, while the observers of retarded egress must be placed near *e′*. The amount of acceleration or retardation will depend on the distance from *c c′*, a station at any point on any one of the parallels of fig. 23 having equal acceleration or retardation (according as the parallel is nearer *e* or *e′*).

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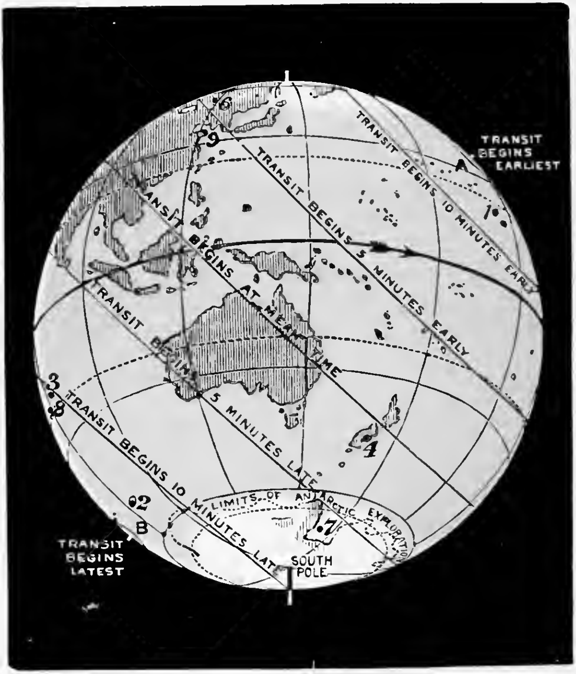
[1] The acceleration or retardation at a station for observing ingress manifestly varies as the distance of the station from the plane of the great circle having *i* and *i′* as poles; and similarly for retarded ingress, and for accelerated and retarded egress. Or in other words, the acceleration or retardation varies as the cosine of the arc-distances from *i* or *i′* *e* or *e′*.

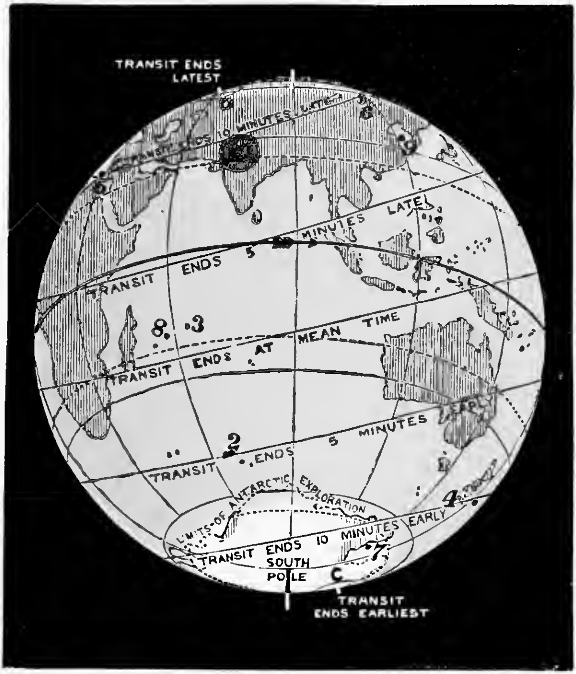
PLATE XII.



SUN-VIEW OF THE EARTH AT THE BEGINNING OF THE TRANSIT OF 1874.

1. Station at Hawaii.
2. " " Kerguelen Island.
3. " " Rodriguez.
4. " " New Zealand.
6. " " Nertschinsk.
7. " (proposed only) at Possession Island.
8. " " Mauritius.
9. " in North China.

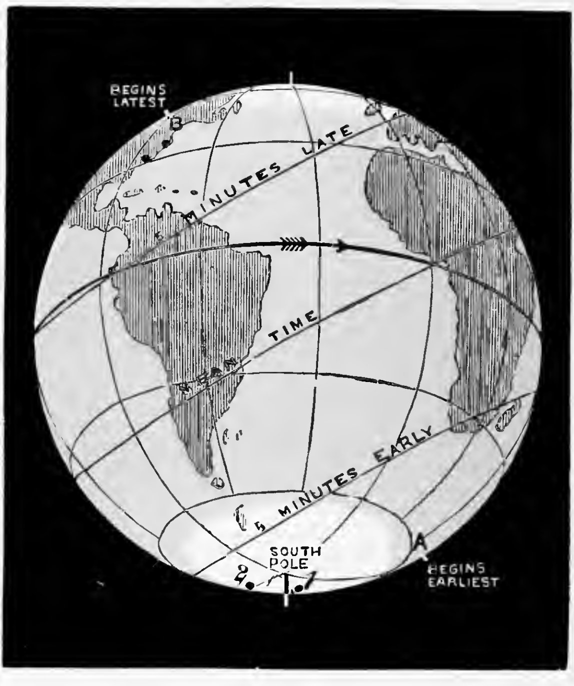
PLATE XIII.



SUN-VIEW OF THE EARTH AT THE END OF THE TRANSIT OF 1874.

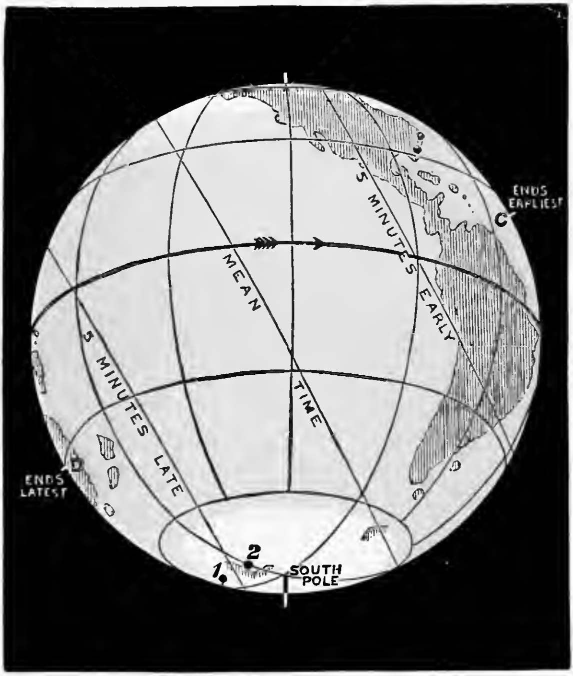
2. Station at Kerguelen Land.
3. „ „ Rodriguez.
4. „ in New Zealand.
5. „ at Alexandria.
6. „ „ Nertschinsk.
7. „ (proposed only) at Possession Island.
8. „ „ Mauritius.
9. „ in North China.
10. The North Indian Region (now occupied).

PLATE XIV.



SUN-VIEW OF THE EARTH AT THE BEGINNING OF THE TRANSIT OF 1882.

1. Sir G. Airy's proposed station at Repulse Bay.
2. " " on Possession Island.



SUN-VIEW OF THE EARTH AT THE END OF THE TRANSIT OF 1882.

1. Sir G. Airy's proposed station at Repulse Bay.
2. „ „ on Possession Island.

These parallel lines of the figure are circles on the globe of the earth, and e and e' are the poles of these circles.

Similar remarks apply to the case of the transit of 1882, illustrated by figs. 24, 25, and 26, Plate X., and by Plates XIV. and XV. The reader is recommended to go over the last three paragraphs afresh, using these last-named figures and plates.

It is easily seen that Delisle's method is applicable in every possible case. The poles of accelerated and retarded ingress and egress lie always (as i, i', e, and e') at the edge of the illuminated hemisphere, and stations an always be found near to these points, and in sunight. Nor do the conditions of success depend at all pon the position of the chord of transit. We see ndeed that in the case of a short chord as in fig. 21, he difference of time between ingress at i and i', or gress at e and e', is greater than in the case of a longer hord as in fig. 24; for $P_1 P_2$ and $P_3 P_4$ are greater in ig. 21 than in fig. 24. But it is easily seen that this dvantage in the case of the shorter chord is counteralanced by the slowness with which Venus crosses he sun's edge.[1] Of course the more slowly Venus seems cross the sun's edge the more difficult it is to deterine the true moment of contact whether at ingress or gress, complicated as contact is by the phenomena of lack-drop formation.

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[1] Plate XI. shows us that from h to the centre of Venus's disc at i is eater than from s to the centre of her disc at i'. In fact this slowness crossing corresponds *exactly* to the lengthening of the distances $P_1 P_2$ d $P_3 P_4$.

Delisle's method is then always applicable, a always under similar conditions. It is otherwise wi Halley's method. Let us briefly consider this meth in the approximate manner already used for Delisl method.

To apply Halley's method both the beginning a end of transit must be seen. Now *i i′*, fig. 22, & enlarged into a sun-view of the earth as in Plate XI (for the case of the transit of 1874), shows the face the earth in sunlight when transit begins, while *e* fig. 23, similarly enlarged, shows the face of the ea in sunlight when transit ends. We must select statio common to both these projections or sun-views of t earth in the case of any transit we are dealing wi For example, in the case of the transit of 1874, we s that such a station as 1, near A in Plate XII. (one the Sandwich Islands), though excellent for observi the beginning of the transit by Delisle's method, co not be employed for Halley's, because this station h already passed to the dark side (in other words, the s has set there) before the end of the transit when t face of the earth pictured in Plate XIII. was turn sunwards. Again, the station marked 5 in Plate XI (Alexandria) was an excellent station for seeing the e of the transit by Delisle's method, but it could not employed for Halley's, because it was on the dark si of the earth (in other words, the sun had not risen), the beginning of the transit, when the face of the ea pictured in Plate XII. was turned sunwards. But any station in Australia, for example, the whole tran could be seen, and therefore Halley's method could be employed there so far as visibility of transit was concerned. It remained, however, to select among stations where both the beginning and end could be seen, those particular stations where the transit was considerably lengthened and shortened in duration compared with the mean transit (transit of Venus's centre supposed to be seen from the earth's centre). In the case of Delisle's method, whether applied to ingress or to egress, we had two poles (*i* and *i′* for ingress, *e* and *e′* for egress), and could estimate the value of any station at once by referring its position to these poles.

We have now to inquire whether there are any corresponding Halleyan poles—a pole of lengthened duration, and another pole of shortened duration. I believe Encke was the first to point out that there are such poles, and to give an analytical proof of the fact; but the following simple geometrical demonstration is, so far as I know, original.[1]

The parallel lines across the disc of the earth in fig. 22, represent circles on the earth having *i*, *i′* as

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[1] Prof. Adams mentioned the fact that there are such poles, and indicated their position, at a meeting of the Astronomical Society at which I was present—March 1873, if I remember rightly. I submitted to him a day or two after the demonstration given in the text, and in his reply he remarked that the demonstration was the geometrical equivalent of the reasoning by which he had been led to recognise the existence and position of the Halleyan poles, as well as the law according to which the lengthening or shortening of the duration varies with distance from the poles. Subsequently I learned from M. Dubois' work, already often referred to here, that Encke had anticipated Adams in recognising these relations.

poles, and corresponding to times of ingress successively later and later, by equal intervals, as the parallel is farther and farther from i. Now if we suppose the globe of the earth rotated about an axis $i\,i'$, these parallel circles will still appear as parallel and equidistant straight lines. In fact, so long as the points i and i' are on the edge of the visible disc the parallel circles will appear as equidistant parallel lines. Similar remarks apply to the parallels in fig. 23; so long as e and e' are on the edge of the visible disc these parallel circles will appear as parallel and equidistant lines. Let us suppose, then, that the globe of the earth is so placed with respect to the observer that all four points i, i', e, and e' are on the circumference of the visible disc. This is clearly possible, for $i\,i'$ are extremities of one diameter, $e\,e'$ those of another diameter of the sphere, and any two diameters must lie in one plane, which plane intersects the sphere in a great circle; so that we have only to place the sphere so that this great circle shall form its apparent outline, to have i, i', e, and e' (the extremities of two diameters of this great circle) on the outline of the visible disc.

Now let fig. 32 represent on an enlarged scale the face of the globe thus brought into view, I_a and I_r corresponding to the points i and i', while E_a and E correspond to the points e and e'. Also the number of parallels has been doubled to make the illustration more complete, the maximum acceleration and retardation at ingress and egress being divided into ten equal parts corresponding to the ten equal spaces on either side of the lines $e\,e'$ and $c\,c'$. For convenience of explanation this maximum is regarded as 10 minutes, which is a little short of its value in the transit of 1874 and a little in excess of its value in the transit of 1882. Now consider any point a where a parallel of one system crosses a parallel of the other system. Since a lies on the fourth parallel from $c\,c'$ towards $\mathrm{I_a}$, the ingress is accelerated by 4 minutes, and since a lies on the third parallel from $c\,c'$ towards $\mathrm{E_r}$, the egress is retarded by 3 minutes. On the whole, therefore, the duration of the transit at a is 7 minutes greater than the mean. At the point b where the next parallel *from* $\mathrm{I_a}$ and the next *towards* $\mathrm{E_r}$ intersect, ingress is

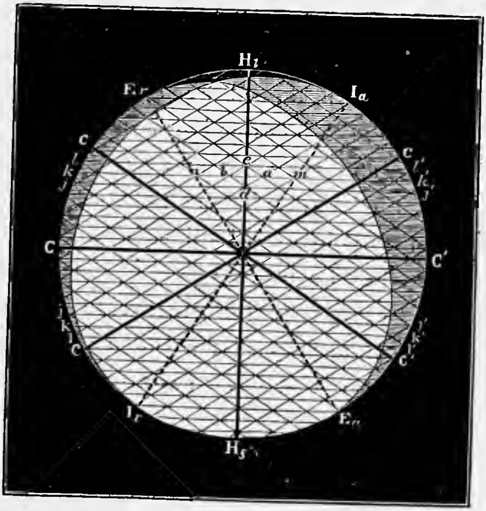

Fig. 32.—Illustrating the position of the Halleyan poles between the Delislean poles.

accelerated 3 minutes, and egress is retarded 4 minutes; so that the duration here, as at *a*, is 7 minutes greater than the mean. At the point *m* the ingress is accelerated 5 minutes, and egress retarded 2 minutes; so that at *m* also duration is lengthened 7 minutes. The same is the case at *n*. These points *m*, *a*, *b*, and *n*, lie manifestly on a straight line which produced either way to *k* and *k′* passes through other points of intersection of our two systems of parallels, at which points, by the method of reasoning already applied to *a*, *b*, *m*, and *n*, the duration is lengthened by the same number of minutes. At *e*, the lengthening is similarly shown to be 8 minutes, and the same at all points on the line *l e l′*. At *d*, the lengthening amounts to 6 minutes, and the same at all points on the line *j e j′*. Along C O C′ the duration has its mean value. And at any point on j j′, k k′, or l l′, on the other side of C O C, the transit is shortened by six, seven, or eight minutes, respectively. So with other cases for all points over the disc C O C′.

Now very little knowledge of geometry is required to show that these lines *j j′*, *k k′*, *l l′*, j j′, k k′, and l l′, and the other lines of fig. 32 similarly obtained, form a series of parallel equidistant lines; these lines being parallel to C O C′, the bisector of the angles E_r O I_r and I_a O E_a. These parallel lines are parallel circles on the sphere, having for poles the two points H_1 and H_s the middle points of the arcs I E_r and I_r E_a. The farther one of the dotted parallels is from C O C′ towards H_1 the greater is the lengthening of the transit, and the farther such a parallel is from C O C towards H_s the greater is the shortening of the transit. The absolute maximum duration is at H_l, and the absolute minimum is at H_s. These points, then, are the Halleyan poles, and any one of the parallel circles having these points as poles indicates the position of stations at which the lengthening or shortening is in proportion to the distance of the plane of the circle from the plane of the great circle C O C′, towards H_l or H_s respectively.

But while the Halleyan poles and circles correspond thus geometrically with the Delislean poles and circles, there is one important difference. A Delislean pole for any phase is a point where the sun can actually be seen at that phase. But this is not necessarily the case with a Halleyan pole. The northern Halleyan pole for example, in the transit of 1874, is, by what has just been shown, the point midway between the A of Plate XII. and the D of Plate XIII. D being still on the darkened side of the earth at the time pictured in Plate XII., and A having passed to the darkened side at the time pictured in Plate XIII., it is manifest that the middle point of an arc from D to A must also lie on the darkened side at both these epochs; and therefore the northern Halleyan pole, though geometrically the point where transit lasts longest, is in reality a point where neither the beginning nor the end of transit can be seen. On the other hand, the southern Halleyan pole in 1874, is in sunlight throughout the whole transit, as we see by noting that the points B and C of Plates XII. and XIII. are themselves in sunlight throughout the transit, and that therefore the point midway between them must be so. The relations here described are those illustrated in fig. 32, where the dark lune $\text{I}_a\ c\ \text{I}_r$ represents a part of the earth where the beginning of the transit is not seen, the dark lune $\text{E}_r\ c\ \text{E}_a$ representing a part where the end is not seen, and H_1 lying on a part where these lunes overlap, on which therefore neither the beginning nor end of the transit can be seen.

The reader will find no difficulty in making a corresponding construction to illustrate the transit of 1882. In fact, so far as the parallels are concerned, fig. 32 will represent the case of the transit of 1882 very nearly, for we see from Plates VI. and VII. that the distance between the two northern Delislean poles, and therefore between the two southern, is nearly the same in both transits—in other words, the arcs corresponding to $\text{I}_a\ \text{E}_r$, $\text{E}_a\ \text{I}_r$ in fig. 32 are nearly right for the transit of 1882. But the darkened lunes must have the position they assume when fig. 32 is inverted; for we see from Plates XIV. and XV. that while the northern Halleyan pole is in sunlight in 1882, the southern is on the darkened hemisphere.

But besides that one Halleyan pole or the other is so placed that no part of the transit can be seen from it, the circumstances of different transits vary as respects the advantages offered by Halley's method.

For example, take H_s the accessible Halleyan pole in such a transit as that of 1874. We see that at this point the ingress is retarded and egress accelerated by the maximum acceleration or retardation, less only about $1\frac{2}{3}$ tenths, so that the shortening is less than the sum of the maximum acceleration and retardation by only $3\frac{1}{3}$ tenths of either. But if E_a and I_r were farther apart the shortening of the transit would not be so great. This is seen from fig. 33, which illustrates the conditions of the transits of 1761 and 2004. Here E_a and I_r are farther apart than I_a and E_r in fig. 32; the southern Halleyan pole H_l is in this case the inaccessible one. We note first, that owing to the greater distance between I_a and E_r the point of nearest approach to the pole H_l is still a long way from that pole. Moreover, we see that at H_s in fig. 33, the retardation of ingress

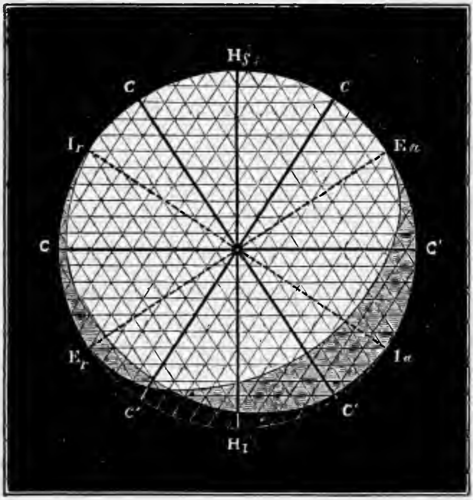

Fig. 33.—Illustrating a case unfavourable for Halley's method.

and the acceleration of egress are less than the maximum by $4\frac{1}{2}$ tenths, so that the total shortening is less than the sum of the maximum acceleration and retardation by fully 9 tenths of either. If the interval in time corresponding to the space between successive parallels were 1 minute, as we before for convenience assumed it, then in the case illustrated by fig. 32, the total shortening at the sunlit Halleyan pole H_s would amount to nearly 17 minutes, whereas in the case illustrated by fig. 33 the shortening at the sunlit Halleyan pole H_s amounts to little more than 11 minutes.

Thus, apart from geographical considerations, which may in some cases be of paramount importance, the applicability of Halley's method depends principally on the arc-distance between the two Delislean poles in the northern hemisphere, which of course is equal to the distance between the Delislean poles in the southern hemisphere.

This seen, it is easy to perceive that the first transit of a pair separated by eight years will be less suitable than the second.

First take a pair of December transits like those of 1874 and 1882—transits when Venus is at her ascending node. In this case the first transit always carries Venus north of the sun's centre, as along *b b′* in Plate XI., while the second carries her south of the sun's centre as along *s s′*; for this being her ascending node, and the second transit finding her, as already explained, less advanced in her orbit, she is beyond her ascending node at the first transit and behind that point at the second transit—that is, in north latitude in the former case, and in south latitude in the latter. Accordingly, the centre of Venus's shadow-cone passes north of the earth as in fig. 21 in the case of the earlier transit of a pair at the ascending node, and south of the earth as in fig. 24 in the case of the later transit of such a pair. Thus the first contact is in the north-eastern quadrant as at *i* in fig. 22, and the last in the north-western as at *e′* fig. 23; and the point *i* on the earth having been carried round by the earth's rotation to the darkened side and (remembering the position of the earth's axis) on a course giving it a greater distance from *e* than it would have if the rotation were round an axis N S, we have the two Delislean poles farther apart than they would be but for the inclination of the earth's axis. Or we might have deduced the same result by considering the two poles *i′*, fig. 22, and *e*, fig. 23; for we see that the motion of *i′* along its upward-bowed latitude-parallel is such as to give it a greater distance from *e* than it would have if the axis of rotation were N S. But in the case of the transit of 1882, we see that *i′*, fig. 25, the place of second contact, is brought by rotation *nearer* to *e*, the place of third contact,[1] than it would be if the rotation were around an axis N S; or we may infer the like by considering the relative motion of the northern Delislean poles *i* and *e′*.

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[1] The reader should note that the effects here considered depend entirely on the tilt of the earth's axis.

The position of the axis of rotation is then unfavourable to the earlier transit of a pair occurring in December.

It will be easy for the student to apply similar reasoning to the case of transits occurring in June, as illustrated by figs. 27, 28, 29, 30, and 31; and it will be found in these cases also the rotation brings the Delislean poles (the northern pair or the southern pair) closer together, *cæteris paribus*, in the case of the later transit of a pair than in the case of an earlier transit.[1]

But another circumstance clearly affects the distance of the two northern, as of the two southern, Delislean poles. If the transit chord be short as in the case illustrated by fig. 21, the points i and e' (reference is now made to the small disc of fig. 21) will clearly be nearer together than where the transit chord is longer, as in the case illustrated by fig. 24; for the shorter the transit chord the greater is the angle enclosed between the intersecting arcs I i and E$'$ e'. Hence shortness of duration by tending to bring the two northern and the two southern Delislean poles close together renders a transit more favourable for the application of the method of duration.

To see, lastly, how geographical considerations enter into the discussion of this problem, compare Plates VI. and VII., illustrating the transits of 1874 and 1882. It will be seen that the northern or

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[1] The same is proved in another way in 'The Universe and the Coming Transits,'—see also pp. 158, 159 of the present work; and in yet another way at pp. 38 and 39 of my treatise on the 'Sun.'

darkened Halleyan pole is nearly as far from the neighbouring sunlit region (for the whole transit) in the case of the former transit, as the southern darkened Halleyan pole is in the case of the latter transit. But the region where such approach would have had to be made in 1874 was altogether accessible, though doubtless bleak and cheerless during the northern winter prevailing there when transit occurs; whereas the sunlit region nearest to the southern Halleyan pole in 1882 is the inaccessible antarctic continent. Keeping away from that continent and within the space defined by the lines $a' b'$, $c' d'$, which show where the sun is ten degrees high at ingress or egress, there is absolutely no spot to be occupied which is near enough to H′ to be worth the trouble of journeying thither. In both cases the region around the sunlit Halleyan pole affords many good stations, though the transit of 1874 was not in this respect comparable with that of 1882; but the absolute absence of any southern station whatever in 1882 where the duration of transit is usefully lengthened, causes the method of durations to be wholly inapplicable on that occasion.

Thus far we have for simplicity considered the centre of Venus, or we may be said to have regarded Venus as a point. It is easy, however, to see what modifications are introduced when we take into account the fact that Venus is a globe. Thus, instead of a double cone, such as $s\ s'\ \text{V}\ v\ v'$ in fig. 20, having the centre of Venus at its vertex, we must consider two double cones such as are shown in fig. 34, each enveloping both Venus and the sun, but one having its vertex outside the path of Venus (giving the shaded cone of the figure) and the other having its vertex within the path of Venus. It is manifest that any observer on the surface of the last-named cone, as for example, at v', will see Venus touching the sun on the outside, *i.e.* in external contact; for the line of sight v' V s' touches both Venus and the sun, but *on opposite sides*, and is therefore directed to a point of contact *on opposite sides of which* the discs of Venus and the sun lie. On the other hand, an observer on the surface of the shaded cone, as on the prolongation of s V, will see Venus just within the sun's disc, or in internal contact; for the line of sight touches both the sun and Venus *on the same side*, so that it is directed to a point of contact *on the same side of which* lie both the discs of Venus and of the sun.

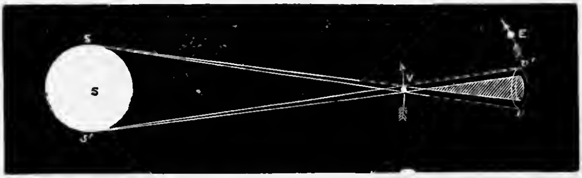

Fig. 34.—Illustrating internal and external contacts.

We see then that taking the two concentric circular sections v v' fig. 34, we have only to substitute *external contact* and *internal contact* for what was said of the *passage of the centre of Venus* in the former case.

In order, however, to still further familiarise the student with these fundamental relations, I shall give an independent description of the relations presented in fig. 34, modifying into a more convenient form the explanation of the actual circumstances of the passage of the section of Venus's shadow-cone (for so the $v\,v'$ of both figs. 20 and 34 may be regarded) over the less swiftly advancing earth.

If an observer were carried through the double cone shown in fig. 34 beyond Venus, he would see the following successive phenomena. When he came to the outer surface Venus would be in exterior contact; as he passed on to the inner surface Venus would enter more and more on the sun's disc, until when he reached the surface she would be in interior contact. Then as he travelled on through the inner cone Venus would seem to cross the sun's disc, and she would just touch it on the inside when our observer reached the surface of this inner region on his passage outwards. Next, as he passed onwards to the surface of the outer region, Venus would be seen crossing the edge of the sun's disc. And lastly, as he passed that surface he would again see Venus in exterior contact, the transit thereupon coming to an end.

During a transit of Venus the earth does actually pass in such a way through these regions; or rather these regions overtake and pass over the earth.

Since the cones overtake the earth in the direction shown by the arrows, we may consider that the earth passes through the cones in the contrary direction.

Suppose $v\,v'$ (fig. 35) to represent the same section of the outer cone as $v\,v'$ in fig. 34; $\mathrm{V}\,\mathrm{V}'$ the section of the inner cone; and E (fig. 35) the earth, as shown at E in fig. 34. Then *v v′* is really moving towards the left; but we are to suppose that E is moving towards the right through *v v′*. Furthermore, if Venus is near an ascending node, as she will be during the approaching transits, we must suppose the earth to pass descendingly along such a course as E E′ through the region *v v′*. The actual course, both as respects position and direction, is determined from the calculated elements of the transit. With this calculation we need not here concern ourselves.[1] The figure shows

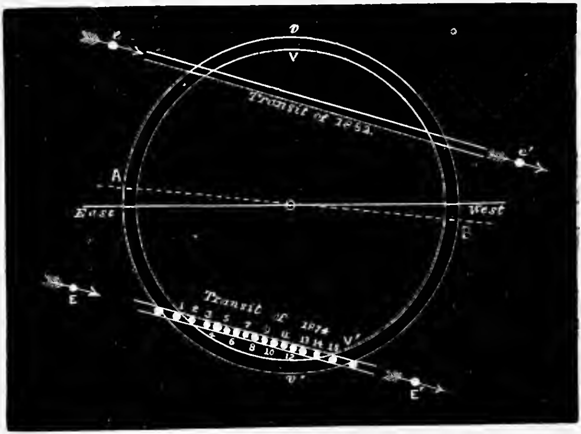

Fig. 35.—Illustrating internal and external contacts.

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[1] As to the size of *v v′* and V V′ compared with that of the earth, it is easily seen from fig. 34 that O *v* is greater and O V is less than the radius calculated in the note, p. 114, for the centre of Venus, by the radius of Venus increased in the proportion that the earth's distance from the sun exceeds the distance of Venus from the sun.

the course actually traversed by the earth in 1874 and 1882.

Now, taking the earth through *v v′* for the 1874 transit, let us consider the various critical points, so to speak, of her course. When she first touched the outer circle *v v′* external contact had begun at that point of the earth which first reached this circle. She passed on, falling more and more within *v v′*, until she was just wholly within. All this time external contact was taking place wherever the outline *v v′* intersected the earth's disc; at parts within that line Venus was seen partly within the sun's disc, and at parts outside of it external contact had not yet taken place. When the earth had passed wholly within the circle *v v′*, external contact had taken place at all parts of the visible hemisphere. But as at this time no part of the earth had reached the circle v v′,[1] internal contact had nowhere commenced. In other words, Venus was not yet fully upon the sun's disc as seen from any part of the earth.

Now, this part of the earth's motion is not illustrated in fig. 35, because external contacts and the passage of Venus across the sun's outline are not phases to which the observers of transits pay great attention. We now come to the important phases.

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[1] The distance between the circles *v v′* and v v′ is obviously greater than the earth's diameter, if we consider how the two circles *v v′* and v v′ are obtained. For the diameter of Venus is very nearly equal to the earth's; so that the diverging lines from *s* or *s′* (fig. 34) are already separated at v by a distance nearly equal to the earth's diameter, and therefore at *v* or *v′* are wider apart.

When the earth just reached the inner circle V V′, interior contact had just begun at the point on the earth which first touched this circle. Here, then, earliest of all, internal contact began, and there occurred at this point the phenomenon called by astronomers *first internal contact most accelerated.* The earth was then in the position numbered 1 in fig. 35.

She passed on, the outline V V′ encroaching more and more over her face until she was wholly within this outline or in position 2. All this time internal contact was taking place wherever the outline V V′ intersected the earth's disc. At parts of the earth within that line internal contact had passed, or Venus was already fully upon the sun's disc. At parts of the earth outside that line Venus still broke the outline of the sun's disc. When the earth was at 2, internal contact had taken place for all places on the earth's illuminated hemisphere. This contact took place latest of all at that point on the earth's surface which at this moment touched V V′. It was here, then, that there occurred the phase which astronomers call *first internal contact most retarded.*

Then the earth passed onwards through the positions shown severally along her track in fig. 35.

As the earth passed out of the spaces V V′, *v v′*, similar phases occurred in reverse order. We need note only the positions numbered severally 14 and 15. The first shows where the earth first reached V V′, and the point on her surface which first touched V V′ is the place where occurred the phase called *second internal* *contact most accelerated*; while 15 shows where the earth just passed clear of V V′, and the point on her surface which was the last to touch V V′ was the place where the phase occurred called *second internal contact most retarded.* The circumstances of the progress of the earth from one position to the other precisely corresponded to those already considered in dealing with the earth's motion from 1 to 2, only they took place in reverse order.

Plate XVI. illustrates the progress of the earth within Venus's shadow-cone during the transit of 1874, through the positions marked 1, 2, . . . 14, 15, in fig. 35. The path of the earth in this figure is, for convenience of engraving, broken up into three parts, shown in fig. 36, and the earth is represented at each part of her progress, precisely poised and rotated, as she would have appeared if she could have been viewed from the sun during the course of the transit of 1874.

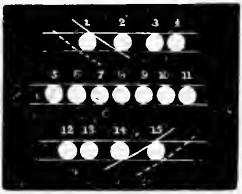


Fig. 36.—Explaining Plate XVI.

Leaving the cross-lines out of consideration for the present, let the student study this plate, interpreting it by reference to figs. 34 and 35, and he will be able to form more exact conceptions of the real relations presented during the transit than he could from a long and recondite explanation. He sees in the first picture of the earth those regions whence the beginning of the transit was visible. He sees in the last those regions where the end was visible. Those parts of the earth which appear in both these views are those from which the whole transit was visible. And, finally, those parts which do not appear in either of these views (nor, therefore, in any of the fifteen) are those whence no part of the transit could be seen.

Plates XVII. and XVIII. represent the earth as supposed to be seen from the sun at the beginning and end of the transit.

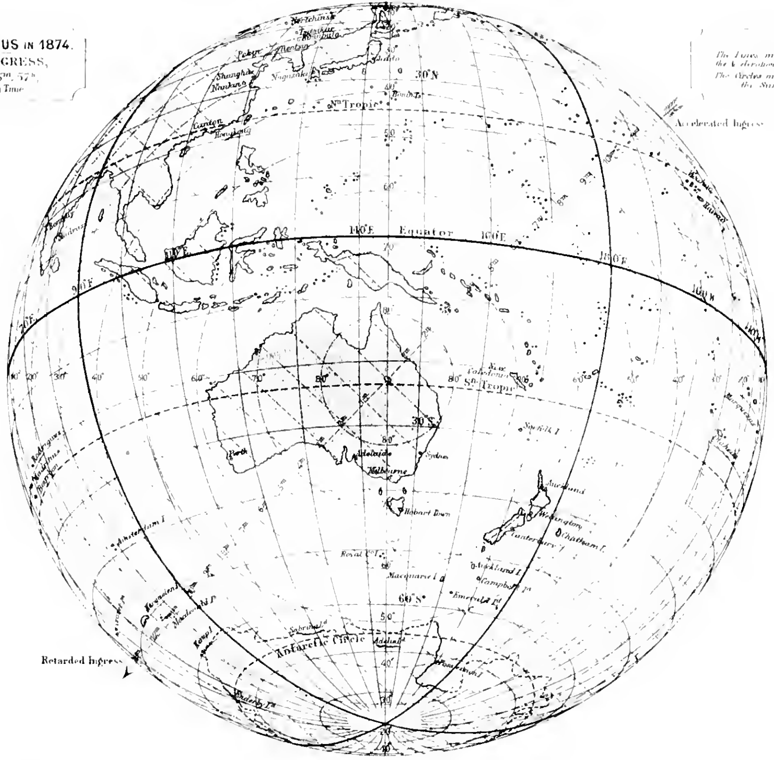
Of these views the first represents the earth as she would appear from the sun when *her centre* was just crossing the circle v v′ of fig. 35 at ingress, and the second represents her as she would appear when her centre was just crossing the same circle at egress. So that the first corresponds to an epoch between those represented in the *first* two earth-pictures of the folding plate, while the second corresponds to an epoch between those represented in the *last* two pictures of that plate. The seemingly parallel cross-lines in Plate XVII. represent the encroaching outline of the circle v v′ (fig. 35) at intervals of a single minute of time between the epochs represented by the first two figures in the folding plate. The corresponding cross-lines in Plate XVIII. represent the same outline gradually passing off the earth's face between the epochs corresponding to the last two figures in the

Pl. XVII

TRANSIT OF VENUS IN 1874.

MEAN INGRESS,
Decr. 8th, 14h. 15m. 57s.
Greenwich Mean Time

Note
The Lines marked — indicate the Acceleration and Retardation in Minutes.
The Circles marked indicate the Sun's Elevation.



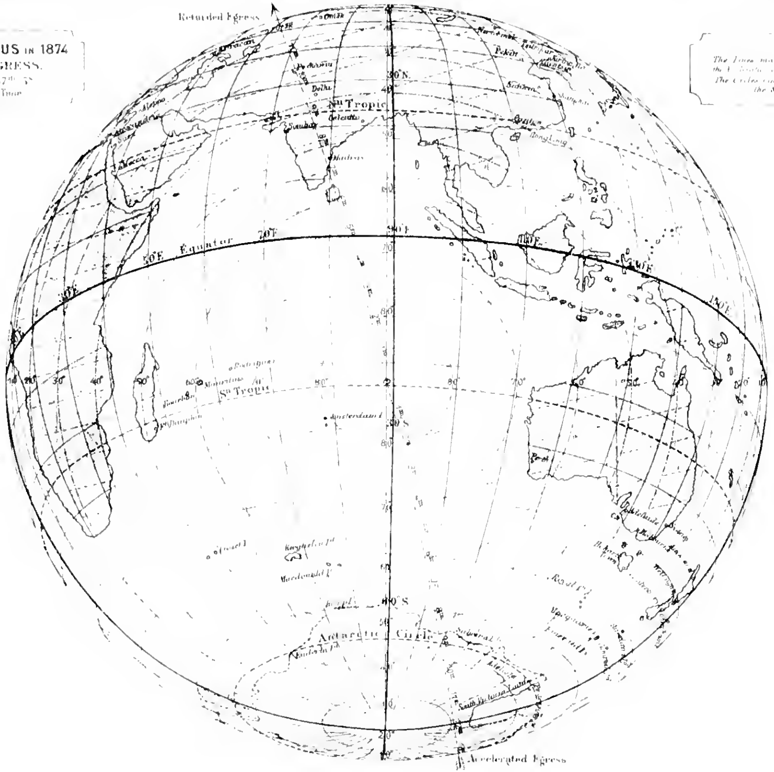
R A Proctor, Delt.

TRANSIT OF VENUS IN 1874

MEAN EGRESS.

Dec^{r}. 8th 17^{h}. 57^{m}. 5^{s}

Greenwich Mean Time



Note

The Lines marked ——— indicate the Accelerations and Retardations in Minutes. The Circles marked ——— indicate the Sun's Elevation.

But now, lastly, it remains to show how the actual progress of a transit as seen from the earth corresponds with the progress of the earth through Venus's shadow-cone as illustrated in figs. 34 and 35. For although the plan of dealing with the problem by considering the passage of the earth through these cones is, on the whole, the most convenient which can be adopted, and especially on this account, that it shows us directly what face of the earth is turned sunwards at the beginning or end or at any other stage of the transit, yet there is something artificial in this way of considering the subject. The student who wishes to know what can actually be seen from the earth seeks for something more than a description of what might be seen from the sun under certain imagined conditions.

A very simple consideration will enable us at once to transpose the relations illustrated in fig. 35 in such a way as to correspond to the actual transits across the solar disc.

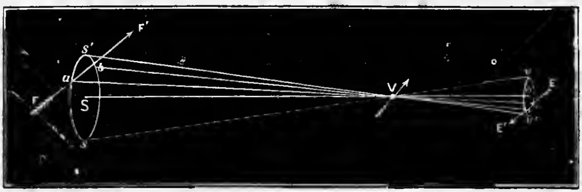


Fig. 37.—Illustrating the connection between the passage of Venus over the sun's face, and the passage of the earth through Venus's shadow-cone.

Suppose S (fig. 37) the sun's centre, *s'* S *s* a diametral plane of the sun square to the line S V O, which forms the axis of the shadow-cones we have been dealing with (for simplicity taking the cone as in fig. 20). Thus *s s′* is a circle directly opposite to *v v′*, the planes of these circles being parallel. (The left-hand halves of the ovals *s′ s* and *v v′* are supposed to be the nearer.) Now imagine a straight line passing through V to the centre of the earth E on one side, and to the circular disc *s s′* on the other. Since the earth's centre carrying this line travels along E E′ athwart *v v′* on the path *i e*, such as is shown in fig. 34, passing slantingly downwards below the centre of *v v′*, it is clear that the other end of the line will travel along F F′, across *s s′*, on the path *a b*, moving slantingly upwards above the centre of *s s′*. If we looked at *v v′* from V, the motion of the earth would be from left to right along *i e*; and manifestly, if we looked at *s s′* from V, the motion along *a b* would also be from left to right. In other words, whereas *a b*, as a projection of *i e*, is inverted, it is not reversed right and left, provided we are supposed to view *v v′* and *s s′*, in turn, from the point V. The chord *a b*, then, so viewed, is a perfect *projection* of *i e*, inverted without being reverted right and left.

And clearly this principle of projection may be extended to all that is pictured in fig. 35, not only as respects motion along the transit chords of 1874 and 1882, but also as respects the sun-views of the earth supposed to be presented by the numbered discs, and actually presented on a much enlarged scale in Plate XVI. The circle *s s′*, of fig. 37, which represents the solar disc, is a perfect projection of *v v′* in this sense, that wherever an observer be supposed to be placed on the circle $v\,v'$, he would see the centre of Venus projected at a point of $s\,s'$ corresponding to his own position, only inverted as respects north and south. And if we imagine a small figure of the earth properly placed on the chord $i\,e$, with correct pose of axis and rightly rotated, to correspond to the time at which the earth actually reaches that part of $i\,e$, then for every point on the sunlit-half of that small globe there will correspond a point on $s\,s'$. If, further, we imagine a straight line extending from V to this globe of the earth on one side and to $s\,s'$ on the other, and that the former extremity is carried along all the outlines of continents and islands on the sunlit-half of the globe, the other extremity will describe on the disc $s\,s'$ an inverted, but not reversed, picture of those continents and seas. Any point in this inverted picture will indicate the point on the sun's disc occupied by the centre of Venus, as supposed to be seen at the corresponding moment by an observer placed at the corresponding point of the earth's globe. So that when once we have constructed such a picture as Plate XVI., giving a series of sun-views of the earth during her passage through the sections $v\,v'$, V V′ (fig. 35), of the shadow-cones shown in fig. 34, we have at once the means of determining the apparent path of Venus's centre across the sun's disc for any station whatever upon the earth. In fact, Plate XVI., held up to the light, inverted, and looked at from behind, pictures the portion of the sun's disc traversed by Venus; the pictures of the earth inverted without reversion are such projections as I have been speaking of; and we have only to dot down the place of any island or town in these successive projections, and to connect the successive dots by a line, to have the path of Venus's centre across the sun's disc as viewed from that island or town during her passage.

Plate XIX. has been constructed in the way here indicated, only that I have thought it better to show projections separated throughout by a quarter of an hour, instead of having internal contacts (most accelerated and most retarded) illustrated specially as in Plate XVI. Plate XX. is intended to explain more clearly the meaning of Plate XIX. It shows the northern half of the sun's disc. Outside and inside this disc are circles, one having a radius exceeding the sun's by Venus's semidiameter, and the other having a radius less than the sun's by the same amount; so that when Venus's centre crossed the outer circle her outline just touched the sun's on the outside, or she was in external contact, while when her centre crossed the inner circle her outline just touched the sun's on the inside, or she was in internal contact. Parts of these circles are shown in Plate XIX. Across the disc five parallel lines are drawn. The central one is the path of Venus's centre supposed to be viewed from the centre of the earth. The line next to the centre, above, shows the path of Venus's centre supposed to be always so viewed from a southerly station as to be thrown as far as possible from the central path on the *northern* side;[1] and the line next to the centre, below, shows the path of Venus's centre as supposed to be always so viewed from a southerly station as to be as far as possible from the path of the central path on the *southern* side. The lines next outside the two last-mentioned mark the boundaries of the track of Venus's *disc* as supposed to be seen from the centre of the earth. And lastly, the outside dotted lines mark the northern and southern boundaries of the tracks pursued by Venus's disc if so viewed that her centre would follow the tracks shown north and south respectively of the central path. *No part of Venus could be seen, from any part of the earth, outside these dotted lines.*

In Plate XIX., the tracks followed by Venus's centre as seen from twelve important stations, are marked in. The student can readily add, either on the plate itself or on a tracing from it, the transit path for any other station. It will be found a useful exercise to trace from Plate XIX. the central path and the outline of the sun's disc, and the path of any stations whether of the twelve dealt with in the plate or such others as the student may desire, and then having cut the picture thus formed into three parts by horizontal lines (where the black spaces fall in the plate) to connect them into one long strip corresponding to the transit band of Plate XX.

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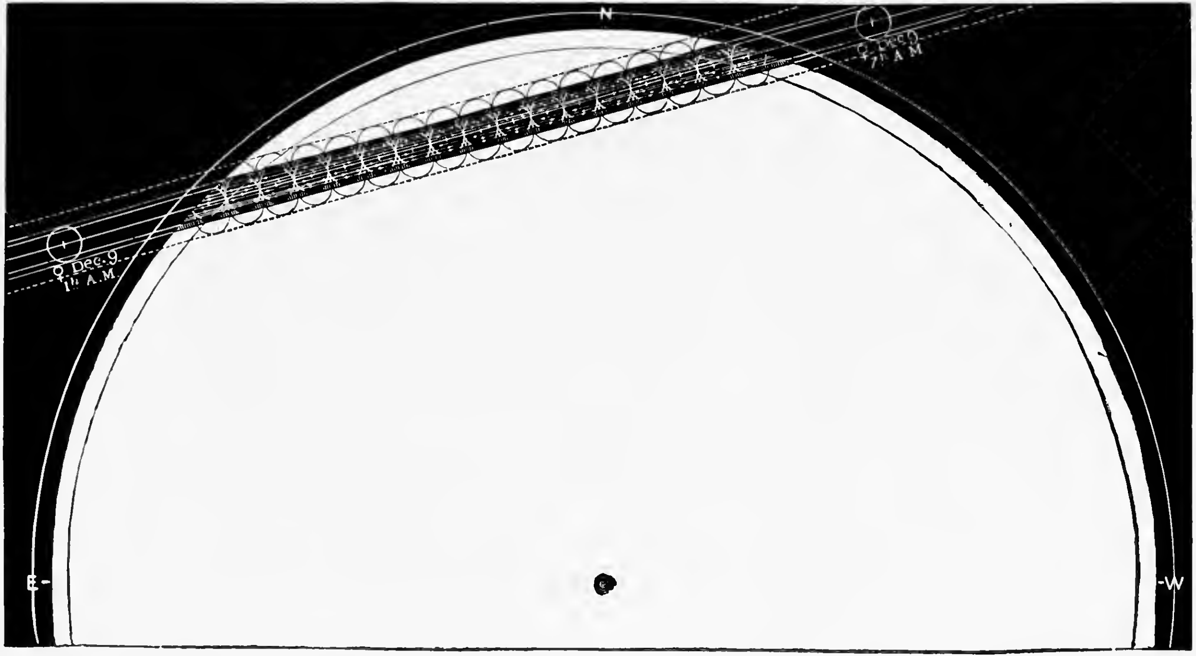
[1] There was no fixed point in the earth where this relation would hold. The observer would have had to be placed at the point of the earth which just touches the southern transit-parallel in Plate XVI., and this was a point continually travelling backwards along a southern latitude parallel. A similar remark applies to the corresponding northerly positions.



Showing the paths of Venus across the Sun's disc during the transit of 1874.
(The transit baud in this drawing may be regarded as a miniature of that shown in Plate XIX.)

It remains only to be added that the process applied n the construction of Plates XVI. and XIX. to illustrate the transit of 1874, can easily be applied to any other transit. Take for instance the transit of 1882. Here a portion of the work has been already done, since Plates XIV. and XV. illustrate the beginning and end, with the position of the circles V V′ of fig. 35. A picture of the space enclosed between the transit chords for 1882 fig. 35, and the circle V V′ ought to be made on such a scale that the distance between the transit chords would equal the diameter of the discs in Plates XIV. and XV. Or, if that scale be too large, then figs. 38 and 39 may be used instead. A series of sun-views can readily be drawn on tracings of the meridians and parallels either of Plates XIV. and XV., or of figs. 38 and 39, corresponding to successive equal epochs (say fifteen minutes apart) all through the transit. These must be arranged in a row as in Plate XVI., in their proper order, and so posed that the central cross-lines (marked MEAN TIME in Plates XIV. and XV., and 0^m in figs. 38 and 39) may cross the track of central transit at the equal angles at which the circle V V′ crosses that track in fig. 35. Then will a picture corresponding to Plate XVI. have been constructed, except that internal contacts will not have been specially illustrated by projections corresponding to these contacts (as most accelerated and most retarded). The picture so drawn, if inverted and looked at from behind (or if inverted and viewed in a mirror), will correspond to Plate XIX., and enable the student to trace the path of Venus's centre as seen from any station whatever on that occasion.

Other transits may be illustrated with equal

TRANSIT OF 1882. (INGRESS.)

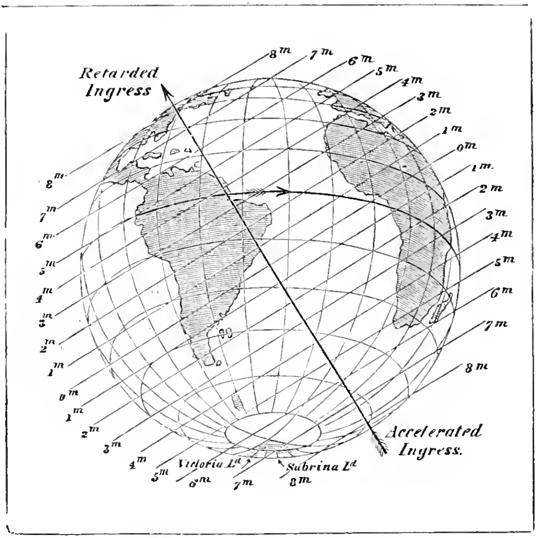


Fig. 38.—Illuminated side of the earth at ingress, Dec. 6, 2h. 15m. 56s. (Greenwich mean time.)

readiness. Nor need the details of the process be any further illustrated by examples, since any student who takes sufficient interest in these matters to attempt the projection of a transit in the manner here applied to the approaching transits, will have sufficiently examined the earlier portions of this chapter to be able to recognise clearly the relations involved in constructions of the kind.

The construction of such illustrative projections as Plates II., III., &c. . . . IX., needs no explanation; for these are simply stereographic polar projections of the earth, upon which various lines and points, obtained by the methods already described, are laid down for convenience of study and reference.

TRANSIT OF 1882. (EGRESS.)

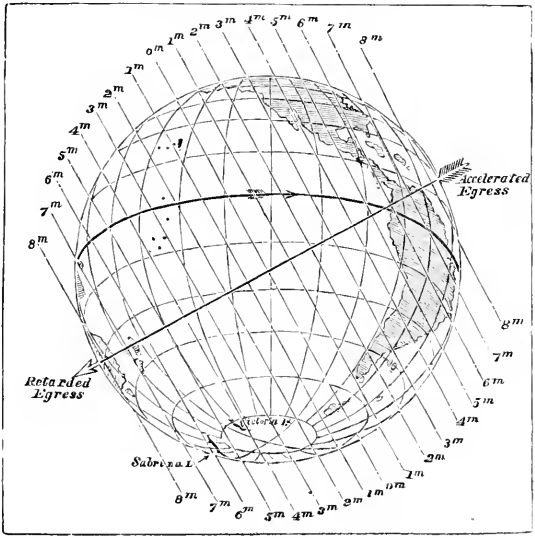


Fig. 39.—Illuminated side of the earth at egress, Dec. 6, 8h. 0m. 32s. (Greenwich mean time.)

CHAPTER V.

THE TRANSITS OF 1874 *AND* 1882.

The discovery that the sun's distance, as determined by Encke from the transits of 1761 and 1769, was considerably in excess of the truth, naturally directed special attention to the transits of the present century. It was in 1857, only three years after Hansen had announced to the Astronomer Royal the correction in the sun's distance resulting from the lunar theory, that Sir G. Airy first called the attention of astronomers to the subject of the approaching transits, and to the inquiry how the opportunities presented by these transits might best be employed. In a lecture delivered before a meeting of the Astronomical Society in May 1857, he examined the various methods available for determining the sun's distance, and ascribing to the observation of Venus in transit the highest value, he considered in a general way the circumstances of the transits of 1874 and 1882. He pointed out that, *cæteris paribus*, the second transit of a pair is superior to the first for Halley's method; but unfortunately failed to observe that special circumstances may modify or even reverse this relation. Although

I have given one demonstration (in the preceding chapter) of the general law and of the fact that the coming transits present an exception to it, it will be well to show here the nature of Airy's reasoning:—

Let fig. 40 represent the face of the earth as supposed to be seen from the sun during a December transit, such as either of the approaching transits. Now, the earth during the transit is moving from right to left, or in the direction shown by the long arrow (the slant of the axis is for simplicity neglected). Her rotation shifts points on her surface in the way shown by the small arrow on the equator, the shift due to this cause being greatest on the equator. This motion manifestly takes place in a sense adverse to that of the earth's motion of revolution, everywhere except at stations on the shaded lune of the disc. Now, Venus transits with the excess of her motion of revolution over the earth's; and anything which tends to reduce the effects of the earth's motion of revolution, increases the excess of Venus's motion—or in other words, hastens Venus in her transit. So that at every point of the unshaded portion of the disc in fig. 40 Venus is hastened, more or less, by the effects due to the earth's rotation. On the contrary, at every point on the shaded portion of the disc Venus is retarded in her transit.

These circumstances affect diversely the two transits of such a pair as we are now awaiting. If fig. 41 represents the sun's disc, the north point being uppermost, then the lines *a b*, *c d*, represent chords of transit in 1874 (*a b* being the chord for a northern, *c d* being the chord for a southern station); and *a′ b′*, *c′ d′* will represent chords of transit in 1882 (*a′ b′* being the chord for a northern, *c′ d′* the chord for a southern station).

It is manifest that in 1874 the conditions affecting the duration of the transit as seen at a northern station were adverse. The chord *a b* was longer, owing to the northerly latitude of the observer; but Venus was hastened on her course, and therefore the lengthening was not so great as it otherwise would have been. We had then one favourable and one unfavourable condition, the latter to some degree cancelling the former. (In some transits of the kind the effect of rotation wholly cancels, or even more than cancels, the effect due to latitude.) The southern station, if taken where,

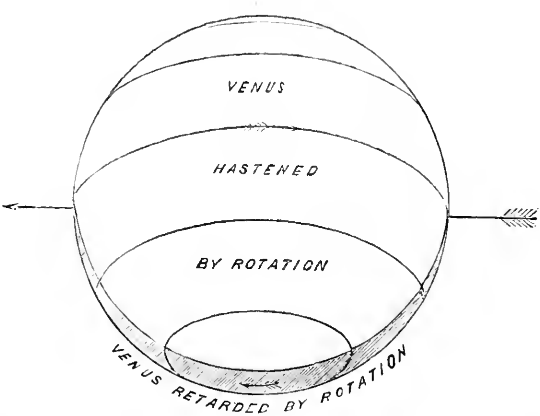


Fig. 40.—Illustrating the effect of the earth's rotation on the progress of a transit.

throughout the transit, the observer was on the portion of the disc represented without shading in fig. 40, would give conspiring effects. The chord of transit *c d* would be shortened, and Venus would be hastened on her course. Hence we had for such a station two favourable conditions. In all we had three favourable conditions and one unfavourable condition—so that if the conditions had been all equal in value we had a balance of only *two* favourable conditions.

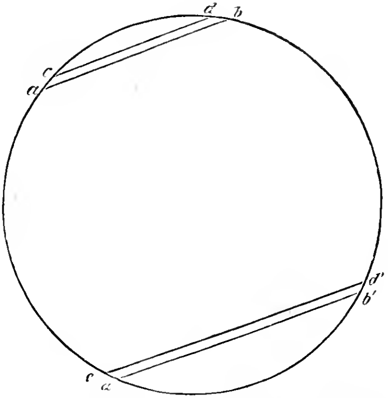


Fig. 41.—Illustrating the effect of the position of transit chords.

On the other hand, in such a transit as that of 1882 we can theoretically secure four favourable conditions. We have at the northern station the shortened transit chord *a′ b′*, and a hastening of Venus—or two conspiring conditions. At a southern station we have the lengthened transit chord *c′ d′*, and by taking a station which throughout the transit lies on the shaded part of the disc (that is, an Antarctic station passing below the pole during the transit hours), we have Venus retarded on her transit path, or again we have two conspiring conditions. In all, then, we have *four* favourable conditions, or twice as many as we obtained for the balance of favourable conditions in 1874.

This is theoretically sound. Moreover, it is quite commonly the case that the effects due to rotation are equivalent to those due to latitude, and that therefore the adverse conditions at a station placed as the northern station in 1874 may be regarded as cancelling each other. In the transit of 1769, for example, the conspiring effects of rotation and latitude were nearly equal. The Astronomer Royal, in his 'Popular Astronomy' (published in 1848, be it noticed), justly assigns to rotation 10 minutes out of the observed maximum difference of duration, 22 minutes. It does not seem rash to infer that he had this result in his thoughts when, after mentioning that the best northern stations would probably not be occupied in 1874, he proceeded to remark (in 1857) that the '*observable difference*' *in the earlier transit would* '*probably not be half of that in* 1882.'

Although the observable difference in 1874 *was really half as great again as in* 1882, yet it mattered very little, at that early epoch, if any mistake of this sort crept into what claimed to be little more than a popular account of the general subject of transits. No one probably considered that the Astronomer Royal attached any weight to the details of his paper of 1857.

In fact, so roughly was the paper prepared that the time of mid-transit in 1874 was an hour wrong—an error not resulting from incorrectness of the tables, for the time of transit of 1882 was very nearly correct. In fact, the paper of 1857, accurate enough for its purpose, had not, and did not seem intended to have, any scientific weight.

But unfortunately, the Astronomer Royal, when next he dealt with the subject, seems to have regarded the transit of 1874 as *demonstrated* by his former rough paper to be unfit for the application of Halley's method. For, in 1864, he published a sufficiently accurate investigation of the transit of 1882, illustrated by projections (corresponding to those forming Plates VI. and VII.) well executed by Mr. J. Carpenter of Greenwich, and in this paper the transit of 1874 was not considered at all. In 1865, he again commented on the circumstances of the transit of 1882 without mentioning the earlier transit. When at length, in 1868, he published what purported to be a detailed description of the circumstances of the two transits, and of the duties not of English astronomers only, but of astronomers generally with respect to the transits, he remarked that Halley's method had been *shown* to fail totally in 1874.

It will serve, I think, to remove misconceptions if I quote here the remarks addressed to the scientific world by Sir George Airy in 1868 respecting the important transits of 1874 and 1882.

'On two occasions,' he writes, ('Monthly Notices,' 1857, May 8, and 1864, June 10) 'I have called the attention of the Society to the transits of Venus across the sun's disc, which will occur in the years 1874 and 1882; and have pointed out that, for determination of the difference between the sun's parallax and the parallax of Venus, the method by observation of the interval in time between ingress and egress at each of two stations at least, on nearly opposite parts of the earth (on which method, exclusively, reliance was placed in the treatment of the observations of the transit of Venus in 1769),[1] fails totally for the transit of 1874, and is embarrassed in 1882 with the difficulty of finding a proper station on the almost unknown Southern Continent.

'The publication of M. Le Verrier's new Tables of Venus, and of Mr. Hind's inferences from them as to the points of the sun's limb at which ingress and egress will take place in each transit (which inferences I have in part verified), has induced me again to examine the whole subject. And, without giving up the hope of using the observation of interval between ingress and egress at each of two stations in 1882, I have come to the conclusion (from all the information which has reached me) that it will be unsafe to trust exclusively to the chance of securing observations on the Southern

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[1] As it has been said (as a correction of my own criticism of the above paper) that Sir G. Airy did not describe the 'method of durations' as failing totally, but only Halley's method, *meaning the method of durations as applied to a nearly central transit,* I invite special attention to his careful wording. The parenthesis removes all doubt as to his real meaning (for the transit of 1769 was far from central); though indeed without the parenthesis the meaning is unmistakable.

Continent; and that, while observations are by all means to be attempted in that manner, it is also very desirable to combine with them observations of the same phenomenon (at one time the ingress, at another time the egress), made at nearly opposite stations whose longitudes are *accurately* known, and recorded in *accurate* local time. This principle being once admitted, the transit of 1874 is, or may be, as good for observations of that class as the transit of 1882; and the selection of localities for the observations must be made with equal care for the two transits.'

He then explains how the maps which illustrate his paper were constructed, and proceeds to discuss the individual maps with reference to the selection of stations for observing the several phenomena. Plate VI. will serve as well as these maps to illustrate what follows:—

I.—'*Stations for observing the Ingress as accelerated by Parallax'—that is, Stations near* I *(Plate VI.), on the Illuminated side of* A B, *but not too near to* A B.[1]

'Owhyhee and the neighbouring islands are excel-

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[1] If 10° be assumed as the lowest elevation at which useful observations can be made, then the stations must not lie within 10° of the circle A B. The arcs *a b*, *c d*, *a′ b′*, *c′ d′*, Plates VI. and VII., indicate this limit for the transits of 1874 and 1882. Thus a station for observing ingress accelerated by parallax (in 1874) should not be anywhere within the zone A B *b a*; so that the best point for observing accelerated ingress would be that point on *a b* which lies nearest to I. Similar remarks apply to observations of retarded ingress near I′, of accelerated egress near E′, and of retarded egress near E, the dotted curves *a′ b′*, *c′ d′*, and *c d* marking the limits outside which stations should be placed.

lent. The factor of parallax[1] is about 0·92, and the sun is at nearly two hours' elevation. There is English society at Woahoo. These islands are just within the tropics. For use of this station the absolute longitude must be accurately determined.

'At the Marquesas Islands the factor of parallax is 0·7, and the sun is nearly as high as at Woahoo. Our neighbours across the Channel have, from the time of Louis XIV., taken an honourable lead in scientific enterprise of every class. I trust that we may rely on them for accurate determination of longitude at Marquesas, and for accurate observation of the ingress in 1874.

'The desert Aleutian Islands can scarcely be recommended, although the factor 0·8 for the westernmost of them, where the sun is highest, is favourable. But it is very probable that the Russians will soon have established telegraphic communication with the mouth of the Amoor, by which its absolute longitude will be accurately determined; and though the factor is only 0·57, the sun is 15° high, and the station will be valuable.

'On the whole, if the British Government will undertake the accurate determination of longitude of Woahoo, and the careful observation of ingress there in 1874, we may consider that good provision is made for the accelerated ingress.'

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[1] This expression indicates the acceleration or retardation at the station, regarding the maximum acceleration as unity.

II.—‘ *Stations for observing the Ingress as retarded by Parallax*’—*that is, Stations near* I′ (*Plate VI.*), *on the Illuminated side of* A′ B′, *but not too near to* A′ B′. *See note, p.* 163.

‘ The best station, as referred to the test of numbers, is Kerguelen’s Island, where the factor is 0·91, and the sun is 25° high. This island is emphatically known as “ The Island of Desolation.” I know not whether its character is so repulsive, or its utility as a zero of longitude so small, as to make our nautical authorities unwilling to determine its longitude, and to station observers there in 1874. If these difficulties are not thought too great, it will be an excellent position. At Crozet’s Islands the factor 0·98 is very favourable, but the sun is rather low (10° altitude).

‘ The next stations in order of merit are Rodriguez, Mauritius, and Bourbon. Mauritius possesses this claim, that it will be a fairly good station, though not so good as Bourbon, in 1882; in 1874 as well as in 1882, it has this disadvantage, that the sun will be low. If only one longitude can be determined in this chain of islands, it ought to be that of Mauritius; if two can be determined, they ought to be those of Rodriguez (for 1874) and Bourbon (for 1882).

‘ At Madras and Bombay the factors, 0·47 and 0·44, are small; but the value of either station does not depend entirely on its simple factor, but upon the sum of its factor with the factors at the stations under head I. These two observatories, with well-known longitudes, will prove very useful stations.

'With the assistance which we may hope to receive from the British Government, we may consider the observation of the retarded ingress as well secured.'

III.—'*Stations for observing the Egress as accelerated by Parallax*'—*that is, Stations near* E′ (*Plate VI.*), *on the Illuminated side of* C′ D′, *but not too near to* C′ D′. *See note, p.* 163.

'Excluding from consideration the Southern Continent as not to be entertained in our thought without the most absolute necessity, the stations in order of merit are the Auckland Islands, Canterbury, Wellington, and Auckland, in New Zealand (factors ranging from 0·83 to 0·77), Norfolk Island (0·66), Melbourne and Sydney (0·6). I omit Chatham Island, where the sun is rather low. The existence of the observatories at Melbourne and Sydney makes the observation of the accelerated egress almost secure, although, in confirmation, I should much desire to have one station at least on the New Zealand group.'

IV.—'*Stations for observing the Egress as retarded by Parallax*'—*that is, Stations near* E (*Plate VI.*), *on the Illuminated side of* C D, *but not too near to* C D. *See note, p.* 163.

'The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84; and we have, therefore, only to consider the elevation of the sun, leaving to the national Governments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the sun will be rather low, and at Moscow it will be on the horizon. We may, with the utmost confidence, leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends.[1] One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria; but, failing this, I trust that no efforts will be wanting to determine accurately its longitude—a longitude which was in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best

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[1] It cannot but be manifest from the whole tone of this passage that the conditions of the transit for the scientific world, and not for British astronomers only, were intended to be presented in the Astronomer Royal's paper.

of all the stations for observation of the retarded egress.'[1]

TRANSIT OF VENUS, 1882, DECEMBER 6.

FIRST, BY THE METHOD OF ABSOLUTE LONGITUDES.

V.—'*Stations for observing the Ingress as accelerated by Parallax'—that is, Stations near* I′ (*Plate VII.*), *on the Illuminated side of* A′ B′, *but not too near to* A′ B′. *See note, p.* 163.

'Omitting for the present all allusion to the Southern Continent, it will be seen that the best station is Kerguelen's Island, its factor being 0·98, and the sun's elevation (12°) probably sufficient. This circumstance, in addition to its value as explained in the discussion of Plate II., renders it well worthy of attention. At Crozet's Islands the factor is 0·9, and the sun's elevation 23°; abstractedly it is preferable to Kerguelen's Island, but not in quite so great a degree as that in which Kerguelen's Island is superior in list II. The next in value are Bourbon and Mauritius, with factor about 0·78, the sun being higher at Bourbon. On comparing these qualifications with those remarked under head II., the reasons

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[1] The absolute omission of the Indian stations here, though they had been mentioned among those useful for observing retarded ingress, is remarkable, but is readily understood when the Astronomer Royal's maps are examined. North India is nearer to E (Plate VI.) than Alexandria, and has a higher sun.

will be evident for my recommendation that either the longitude of Mauritius or the longitudes of Bourbon and Rodriguez should be determined.

'At the Cape of Good Hope the factor is about 0·62, and the observation there will be valuable.

'The satisfactory observation of the accelerated ingress requires, however, some longitude-determinations.'

VI.—'*Stations for observing the Ingress as retarded by Parallax*'—*that is, Stations near* I (*Plate VII.*), *on the Illuminated side of* A B, *but not too near* A B. *See note, p.* 163.

'Every city near the seaboard of the United States of America, and every important city of Canada, commands this phenomenon most favourably. The lowest factor is 0·95, and the smallest elevation of the sun is 12°. The utmost reliance may be placed on the zeal of our American brethren for observing the ingress. As great facility exists for determining the absolute longitude of any place within the range of American telegraphs (Harvard having been accurately referred to Greenwich), it is unnecessary to look further. Otherwise it might be remarked that Bermuda, Jamaica, and the West Indian Islands, and both sides of Central America, are excellent stations, but requiring determinations of longitude.'

VII.—‘ *Stations for observing the Egress as accelerated by Parallax*’—*that is, Stations near* E (*Plate VII.*), *on the Illuminated side of* C D, *but not too near* C D. *See note, p.* 163.

‘All the American stations mentioned in the last paragraph, from Halifax to New Orleans, and Bermuda and the West Indian Islands, are well situated for this observation, the factors being near 0·85, and the sun’s altitude varying from 4° at Halifax to 32° at New Orleans and Jamaica. The coast of South America also is favourable, from its union with the isthmus to the harbour of Rio de Janeiro. It is believed that efforts have been made for exact determination, in a nautical sense, of the longitude of Rio; it may now be desirable to give to that longitude the utmost accuracy.’

VIII.—‘ *Stations for observing the Egress as retarded by Parallax*’—*that is, Stations near* E′ (*Plate VII.*), *on the Illuminated side of* C′ D′, *but not too near* C′ D′. *See note, p.* 163.

‘Omitting for the present the Southern Continent, this observation will be amply secured by the observatories of Sydney and Melbourne, where the factor is 0·96, and the sun’s elevation 12° to 14°. If, however, the longitudes of the New Zealand stations can be ascertained, they, with factor 0·8 and sun’s elevation 32°, will form a valuable addition.’

SECOND, BY THE METHOD OF INTERVAL BETWEEN INGRESS AND EGRESS.

'On comparing Plates VI. and VII., it will be seen that the North American localities supply, in a manner which leaves nothing to be desired, the demand for stations, at which the ingress is retarded and the egress accelerated, or the whole interval is diminished, by parallax.

'With these, it is necessary to combine one or more stations, at which the ingress is accelerated and the egress retarded, or the whole interval is increased, by parallax. On examining' Plates VI. and VII., 'it will be seen that the only possible method of responding to this demand is by the selection of stations on the Antarctic Continent, in which the observation will be made when the sun is nearly below the Pole.

'In so far as the coast of the Antarctic Continent follows nearly a parallel of latitude, the best position for a station is at 7^h east longitude. The factors would be, for ingress and egress, about 0·95 and 0·68. The sun would be, at each station, about three hours from the sub-polar meridian. But its elevation above the horizon would scarcely exceed 4°, and any alteration of the longitude, with the view of increasing the elevation at one phenomenon, would diminish it at the other.

'Advantage may, however, be taken of the deep southern inlet discovered by Sir James Ross, to the western side of which is given the name of South

Victoria. If a station can be established in latitude exceeding 72° S., it will be preferable, for observation of ingress, to the station in 7^h longitude, and if the expedition could be pushed on to an observing place in the neighbourhood of Mounts Erebus and Terror, that position would be greatly preferable. For observation of egress, it is manifestly far superior; the sun's altitude being about 27°. The factors for the two observations are respectively about 0·78 and 0·58.

'The decision on the choice to be made between these two stations, and the judgment on the facility, or even the possibility, of using either of them, must rest with persons who have had some familiarity with polar, and, if possible, with south polar voyages.'

'In partial correction of some small inaccuracies in these remarks, it may be observed that—

'The ingress, as viewed from the earth's centre, is always a few minutes earlier, and the egress always a few minutes later, than is supposed in the maps.'

'As affected by parallax, the phenomenon is always retarded with ascending sun and accelerated with descending sun.'

'As referred to apparent solar time, the phenomena are slightly retarded.'

'The only phenomena which are critically affected by these corrections are those' of Plates IV. and V., 'and in both the circumstances of solar elevation are rendered more favourable.'

This account of Sir G. Airy's treatment of the two transits would be incomplete without some description of his views as to the occupation of Antarctic stations, or without an account of the opinions advanced in support of his views by authorities whom he had invited to attend the meeting of the Astronomical Society before which the above programme was advanced.

Passing over the Astronomer Royal's remarks in the 'Monthly Notices' for May 1857, and June 1864, I quote from a paper in the 'Monthly Notices' for May 1865, pp. 201–203, called a 'Letter from the Astronomer Royal to the President of the Royal Geographical Society,' and running thus:—

'I have learned, through the public papers, the tenor of late discussions at the Royal Geographical Society in reference to a proposal for an expedition towards the North Pole. I gather from these that the object proposed, as bearing on science, is not so much specific as general; that there is no single point of very great importance to be obtained, but a number of co-ordinate objects whose aggregate would be valuable. And I conclude that the field is still open for another proposal, which would give opportunity for the determination of various results, corresponding in kind and in importance to those of the proposed Northern Expedition, though in a different locality, and would also give information on a point of great importance to astronomy, which must be sought within a few years, and which it is desirable to obtain as early as possible.

'In the year 1882, on December 6, a transit of Venus over the sun's disc will occur; the most favourable of all phenomena for solution of the noble problem of determining the sun's distance from the earth, provided that proper stations for the observation can be found. (It will be remembered that it was for the same purpose that the most celebrated of all the British scientific expeditions, namely, that of Captain Cook to Otaheite in 1769, was undertaken. The British part of the enterprise was perfectly successful; but there have always been doubts of the accuracy of the corresponding observations in Lapland, which render a repetition of the observation very desirable.) In the "Monthly Notices of the Royal Astronomical Society" for June 10, 1864, I have very carefully discussed the circumstances of the coming transit. in reference to the selection of observation-stations. For the northern stations there will be no difficulty; they will be on the Atlantic seaboard of North America, or at Bermuda; all very favourable and very accessible. For the southern stations the selection is not so easy; the observation must be made on the Antarctic Continent; if proper localities can be found there, and if the circumstances of weather, &c., are favourable, the determination will be excellent; if those favourable circumstances do not hold, no use whatever can be made of the transit.' . . . 'The astronomical object of a southern expedition is, I trust, sufficiently explained in the sentences which I have quoted. In the event of such an expedition being undertaken, the precise determinations which I have indicated as bearing on the astronomical question must (from the nature of the case) take precedence of all others. But there would be no difficulty in combining with them any other inquiries, of geography, geology, hydrography, magnetism, meteorology, natural history, or any other subject for which the localities are suitable.

'And I have now to request that you will have the kindness to communicate these remarks to the Royal Geographical Society, and to take the sense of the Society on the question, whether it is not desirable, if other scientific bodies should co-operate, that a representation be made by the Royal Geographical Society to Her Majesty's Government on the advantage of making such a reconnaissance of the Southern Continent as I have proposed; primarily in the interest of astronomy (referring to my official responsibility for the importance of the examination at this special time); but conjointly with that, in the interests, perhaps ultimately more important, of geography and other sciences usually promoted by the Royal Geographical Society.'

In December 1868, notwithstanding the relatively unfavourable circumstances for applying this (Halley's) method to the transit of 1882, and the very favourable conditions under which Delisle's method can then be applied, the Astronomer Royal urged that only three stations should be occupied for Delisle's method in that year, the instruments of the five 1874 expeditions, 'thus set free from two stations,' being required at an observing station on the Southern Continent. 'The choice of station being made,' he said, 'I would not recommend any reconnaissance, but I would propose that an expedition should go direct to the selected point in good time for the observation of the phenomenon. The season is early for South Polar expeditions, and any difficulties produced by ice would probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is a few days' observation to give clock-rate; then the clock times of the two phenomena will furnish all that is required. The first action to be undertaken by the Government,' he proceeds (and I invite special attention to the point), 'is to procure the stock of instruments, and this ought to be done without delay. An observing plant like that' (described in the earlier part of the same paper) 'is not to be obtained in haste, and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships and the selection of officers would probably require much less time.'

It appeared to the naval authorities who followed the Astronomer Royal in addressing the meeting, that the more certain course for achieving the desired result would consist in the preparation of an expedition to winter in Possession Island. I quote the following passages as bearing specially on the feasibility of such an expedition :—

Admiral (then Captain) Richards, Hydrographer to the Admiralty, said: ‘My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that landing a party on Possession Island,’ or one of the islands farther south, ‘would be the most feasible course, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations. Doubtless a year passed in this region would be most profitably employed in adding to our knowledge of magnetism, and various other branches of physical science.’

Admiral Ommanney said, *inter alia*: ‘I fully concur in all that has fallen from the Hydrographer to the Navy, and hope ere long to hear that operations are making for sending out to explore the Antarctic Seas.’

Commander J. A. Davis, who had accompanied Sir James Ross in that most gallant expedition during which Victoria Land was discovered, and who had himself landed at Possession Island, said that ‘he believed there would be no difficulty whatever in again effecting a landing in the same place.’ ‘With regard to the period of the season at which the transit took place, it was to be remembered that the 6th of December was so early that no ships had ever reached the Antarctic Circle by that date; and as it would be necessary to arrange the instruments, &c. preparatory to the observation, he might say that the ships ought to be on the spot at least a month before. This would be the 6th of November, a date altogether out of the question; and as the ships could not winter in the South, the party would necessarily have to land the year before; but with good tents he had no doubt they could pass the winter very comfortably' (this, of course, and what follows, will not be taken strictly *au pied de la lettre*): 'they would have a pleasant prospect before them and plenty of penguins to live on. In comparison with Kerguelen Island and the Crozets,' he proceeded, 'the chances of observing the transit—meteorologically speaking—would be greatly in favour of South Victoria.'

Captain Toynbee also expressed an opinion strongly adverse to the meteorological chances at Prince Edward's Islands, the Crozets, and Kerguelen Land, since their neighbourhood is, he said, 'so far as my experience goes, subject to a great deal of thick weather.'

There were several points in the Astronomer Royal's communication to the Astronomical Society on this occasion which were calculated to attract the attention of those who had followed his former proceedings in connection with the transits of 1874 and 1882. Thus far he alone of all the leading astronomers had publicly dealt with the subject, and there was much in the tone of his preceding papers to suggest that in a sense he guaranteed a sufficient examination of the conditions of the transit to enable astronomers generally, not those of England alone, to await his announcement of what the different scientific nations might be expected to do, and to follow his instructions whensoever such announcement should be made. In the paper of December 1868 he still adopted this tone, while nevertheless it was apparent that he was not treating the subject in an exact manner. For instance, the statement in the very beginning of his paper that the method of duration had been *shown* to 'fail totally,' even if correct in itself—which subsequent examination showed not to be the case—was not in accordance with former papers, in which he had only expressed his opinion that *in all likelihood* it would not be advantageously applicable. Then secondly, the maps accompanying the paper were of the roughest possible description, insomuch that the shapes of the continents and oceans were barely recognisable: nor did these maps extend beyond the parts immediately adjacent to the points corresponding with I, I′, E, and E′ in Plates VI. and VII.; so that if by any possibility (which seemed at that time, however, incredible) Halley's method were available in 1874, the maps could not have shown the fact, though this was precisely the sort of service for which alone maps could have any real value. Thirdly, the necessity for exact accuracy seemed so little to be suspected by Sir G. Airy, that the elements of the transit, given correctly by Hind, were not even congruously employed, the *times* of ingress and egress being these corresponding to *b*, *b′* of Plate XI., while the *positions* were those corresponding to external contact, points which in reality are farther away from *b* and *b′* than are *c* and *c′* respectively.

Whether these circumstances operated with Puiseux, Hansen, Peters, and others who about this time began to inquire into the conditions of the two transits (and sooner or later published correct results), I do not know; but, for my own part, I was led to deal with the subject by the manifest signs of incompleteness in Sir G. Airy's paper. It appeared to me that there was room for a rediscussion of the whole problem, even though, as I fully expected, the general views advanced in that paper should prove to be altogether correct.

Having now ascertained that the subject had not as yet been thoroughly dealt with, I submitted to the Astronomical Society in May 1869 a paper accompanied by six projections (from two of which Plates XVII. and XVIII. have been reduced by photolithography) illustrating the transit of 1874. This paper was published in the June number of the 'Monthly Notices.' In this paper, where it was necessary to call due attention to the changed values of the various stations, I presented these in a tabular form,—Airy's values under head A, those of Puiseux under head B, and my own, which closely accorded in the main with those of Puiseux, under head C. From this paper I quote the following summary of the conclusions therein demonstrated:—

1. '*The application of Delisle's method of absolute time differences.* The relative as well as the absolute values of many stations are affected. Some which had hitherto appeared unsuitable are found to be unobjectionable. Others which seemed good appear unfit. In other cases the relative values of two stations are

so affected that the results of a comparison between them are directly reversed. Lastly, many stations not hitherto thought of in connection with the transit are found to be well suited for the application of Delisle's method.

2. '*The comparison between Delisle's and Halley's methods.* Halley's method' (estimated by Sir G. Airy's own test) 'is found not merely to be applicable with advantage, which is all that can be said of it when central passages are considered, but to be superior to Delisle's—slightly, when reference is made only to such stations as had been hitherto dealt with, noticeably when Antarctic stations are made use of.'

3. '*The comparison between the transits of* 1874 *and* 1882 *with reference to Halley's method.* This comparison shows that Halley's method may be applied much more advantageously to the transit of 1874 than to that of 1882.'[1]

The tables (which are given in abstract at the end of this volume) were followed by these remarks:—

'It will be seen, on a comparison of tables A, B, and C, that the effects of the change of phase are in some cases important. The coefficients of parallax are affected in several instances by more than 0·1 and in two cases by 0·22. In the cases of Crozet Island (Table II.) and Chatham Island (Table III.) solar

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[1] Chapter IV. presents in pp. 128 *et seq.* an abstract of the reasoning by which the applicability of Halley's method in 1874 was demonstrated, and also indicates the places where the method can be most suitably applied. But the tables at the end of the book should be consulted, especially Table V.

elevations are so improved, that these stations, which would have to be rejected if central passage were considered, are shown to be well suited for the observation of internal contacts. The diminution of all the coefficients in Table III., through the change of phase, has an important influence on the value of Delisle's method, so far as egress observations are concerned. It is important to notice, also, that under the heads C in Tables III. and IV. many stations not hitherto recognised as available are included among the best places for observing egress. The Indian stations in Table IV. seem too valuable to be neglected.[1] Peshawur is better even than Alexandria; Delhi is not inferior to the latter station (when solar elevation is considered as well as coefficient of parallax). Bombay, Calcutta, and Madras are also excellent. It may be noticed also that Bombay and Madras, which, when considered with reference to central passage, had seemed suitable places for the observation of retarded ingress, are found to have so poor a coefficient of parallax when reference is made to internal contacts, that it would seem useless to observe ingress there (so far at least as the application of Delisle's method is concerned).

'Of course, it will be impracticable for this country to send observers to more than a certain number of

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[1] Several times during the past year the mistake has been made of stating that I originally advocated North Indian stations for applying the photographic method. The above passage, written before this method had been thought of, contains my first reference to those stations.

stations. But it is not unlikely that besides Russia, France, and England (the only countries specially concerned in the transit of 1874), other nations may care to take part in the solution of the noble problem of determining the sun's distance; and thus it seems advisable that all the stations where there will be any chance of obtaining useful observations, should be tabulated as nearly as possible according to their relative values.'

A discussion followed in which an attempt was made to show that Delisle's method was equal in value to Halley's. Even if this could have been proved, it would have been little to the purpose, since the question was not whether Halley's method was more or less favourably applicable than Delisle's, but whether it was applicable at all. This discussion was carried on in public. A private correspondence arose out of a letter which I addressed to Sir G. Airy, assuring him that my sole wish was to assist in securing what every astronomer agreed was desirable—the best possible utilisation of the opportunities available in 1874 and 1882. Sir G. Airy wrote me a letter, forwarding a copy to Admiral Manners, then President of the Astronomical Society, in which he complained that his paper of December 1868 had been treated as though it claimed to be an exact discussion of the conditions, due allowance not being made for his own statement that it was but a preliminary and comparatively rough investigation of the problem. This led me to believe (mistakenly, as afterwards appeared) that I had taken up the subject too hastily; for Sir G. Airy seemed not merely to promise a thorough analysis of the whole subject, but to imply that this was what had been intended all along, the suggestions made in December 1868 being merely provisional, and the actual arrangements to be proposed to Government depending on the promised thorough investigation.

Nothing could have been more satisfactory than the course thus indicated; and, accordingly, from that time (the summer of 1869) until the summer of 1872, I addressed no communication whatever on the subject of the controversy,[1] either to Sir G. Airy personally or to the Astronomical Society. Nothing, however, was done in this interval except to carry out the arrangements proposed in 1868. Accordingly, in 1872 I wrote to Sir G. Airy, recalling his attention to the promised investigation of the subject. I then learned for the first time that the old arrangements were still adhered to. On this, I made such protest as a student of astronomy, independent of official trammels, might properly (in my judgment then and now) address to the official astronomer to whom the charge of the matter had been left in accordance with ancient custom.

After this protest I allowed yet half a year more

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[1] A paper of mine appeared in the 'Monthly Notices' for January 1870, in which the application of photography to the observation of the transit was dealt with; but this paper bore no reference to the questions which had been raised by me in 1869.

to elapse, and then, nothing having been done, it seemed time to take more earnest measures.[1]

I accordingly resumed the discussion in the 'Spectator' for February 8. By a singular coincidence a powerful paper appeared in the 'Times' of February 13 supporting the views which I had advanced. This paper was commonly, and I believe correctly, attributed to Sir Edmund Beckett. (In his 'Astronomy without Mathematics' he mentions the rumour without contradicting it.) And many believed that the coincidence was not accidental—that is, that the nearly simultaneous appearance of the two papers had been planned beforehand. But this was not the case. Neither had Sir E. Beckett any prior knowledge of my intention, nor had I of his.

In his reply, the Astronomer Royal opposed the use of Halley's method on the ground (i.) that the Russians would probably not occupy Nertschinsk, the station in Siberia (marked 6 in Plates XII. and XIII.) which I had specially recommended, (ii.) that Puiseux had probably abandoned his ideas respecting the use of Halley's method (which ideas, said Airy, 'have not again been promulgated on the Continent'), and (iii.) that no other nations would care to provide for northern Halleyan stations. A few days later, Mr. Goschen, then Secretary of the Admiralty, said that even at

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[1] In the interval events had taken place within the Astronomical Society to which I see Sir Edmund Beckett has thought it desirable to refer in the latest edition of his 'Astronomy without Mathematics.' Those events had no real importance, however, except for the *animus* shown.

those stations already provided for, where, as I had shown, durations could be readily noted, they *would* indeed be noted, but little reliance would be placed on them![1]

But fortunately within a few days news came *that Russia proposed to occupy not only Nertschinsk, but ten other Halleyan stations in Siberia*; that America proposed to occupy three other northern Halleyan stations; Germany two others; and before long it was announced that France would occupy two other northern Halleyan stations.

It was still possible that these energetic proceedings by other nations might be rendered useless by shortcomings on our part; for as yet no adequate provision had been made for southern Halleyan stations, and it was manifest that other nations were looking to England to take a large share in this part of the work. Eighteen northern Halleyan stations were provided for, and as yet there was only one first-class southern Halleyan station, Kerguelen's Land,—and that originally named without the least idea that it was a Halleyan station at all.

The time had come for very plain speaking. Be it noted that if Delisle's method succeeded at each of the selected stations, then the transit would yield very good results; but even then not so good as though

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[1] Sir George Airy soon after wrote to me that there had been some misunderstanding here. It can be easily understood that Mr. Goschen who, of course, had no technical familiarity with the subject, might have misapprehended some statement addressed to him by Sir G. Airy.

Halley's method were extensively applied in addition. For every method successfully applied, and indeed every observation, reduces the probable error in the final result. But there was at this stage a risk that the operations would fail altogether. If, as was possible, Delisle's method were frustrated by bad weather at the Sandwich Islands *or* at Kerguelen Island and neighbouring stations, then ingress observations would fail. If egress observations were frustrated by bad weather at New Zealand and neighbouring stations, *or* at the opposite region, then egress observations would fail. In either case very imperfect results would be attained; but if both events were to happen, then no result at all would be achieved. Now there were eighteen northern Halleyan stations admirably suited to supply a third chance of success by Halley's method if they were but properly balanced in the southern hemisphere, but otherwise valueless. For although, besides being Halleyan, they were also excellent as subsidiary Delislean stations (each having a double chance, because either the beginning or the end would serve for that method), yet the multiplication of northern Delislean stations could not remove the chances of failure on account of the fewness of southern stations. In either respect, whether to balance the northern Halleyan stations as such, or to give new Delislean chances, nothing was at that time promised. Apart from all question of the choice of methods, there was no suitable provision for southern observation. Kerguelen's Land was the only first-class Halleyan southern station yet provided for, and none of the other southern stations could be regarded as high even in the second class. These others—Canterbury Island, Auckland Island, Mauritius, and Rodriguez, stood fairly well in the second class, and that was all that could be said. The only good station, Kerguelen's Land, was one at which all the meteorologists had said that bad weather was far more probable than fair weather. I had already pointed out, as geometrically suitable, Kemp Island, Crozet Island, Macdonald Island, the group of islands of which Campbell Island is the chief, St. Paul's Island, and several others of less value, yet well worth occupying if geographically suitable. But Admiral Richards, without mentioning these islands by name, had in the 'Times' described me as recommending the occupation of places which were 'little better than geographical myths.'

It began to appear as though, after all, nothing would be effected until too late, so that perhaps some time about the year 1877 or 1878 astronomers would be lamenting that the favourable opportunities presented by the transit of 1874 had not been properly utilised.

But at this critical stage a new force appeared on the field, and compelled the Admiralty to retreat from the position they had so bravely defended. The Board of Visitors at Greenwich met on June 7, 1873, and it was there proposed, by Professor Adams, and carried unanimously, that Professor Cayley, who in his capacity as President of the Astronomical Society was Chairman of the Board, should apply to Government 'for the means of organising parties of observers in the Southern Seas, with the view of finding additional localities in the sub-Antarctic regions for observing durations'—that is, for applying Halley's method.

Other nations had not been deterred by the dangers and difficulties which unquestionably have to be encountered in voyages to sub-Antarctic stations. America, inquiring among sealing captains, found that the Crozet Islands could be occupied, and determined to send an observing party to that first-class Halleyan station, as well as to occupy second-rate Halleyan stations in Tasmania, New Zealand, and Chatham Island. The French Government decided to occupy Campbell Island and St. Paul's Island (those inaccessible 'geographical myths'), besides a second-rate southern station at Caledonia Island. Thus already the first-class Halleyan stations had been quadrupled in number, and the second-class largely strengthened. Germany and America both proposed to occupy Macdonald Island, if possible, but found that this island (sometimes called Heard Island) really is almost, if not quite, inaccessible, though an attempt was made to land a party there. England decided to send a second observing party to Kerguelen Island (to be stationed fifty miles from the other). Regarding the four Kerguelen stations as equivalent to at least two separate chances of success, adding the first-class Halleyan stations, Crozet, Campbell, and St. Paul, and the second-class stations, Bourbon, Rodriguez, Mauritius, Hobart Town, Melbourne, Sydney, &c., as well as the first-class Delislean stations in the South, and remembering that all the southern stations were good Delislean stations, while several of the added Halleyan stations were doubly good Delislean stations because serving both for ingress and for egress, it must be admitted that the risk of failure to which I pointed in May 1873 was as completely removed as circumstances allowed.

The North Indian station, Roorkee, which had been first intended only for photographic work, was provided for as a Delislean and Halleyan station.

In the meantime, a new and most important auxiliary method of observing the transit—the photographic method—had been suggested by Dr. De La Rue.

In December 1868 he read a paper before the Astronomical Society containing the following remarks, *inter alia*:—

'The conditions which transits of Venus offer for the determination of the relative position of the sun's and planet's centres are more advantageous than those presented by solar eclipses, inasmuch that it is far more easy to measure directly the distances between the centre of the disc of the sun and that of the image of the planet upon it, than it is to measure the distances between the peripheries of the sun and moon,[1] or the angular opening of the cusps[2] of the partially eclipsed sun. And in transits of Venus any error of observation would not affect the final result nearly so much as in solar eclipses; for example, in the transits

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[1] *Phil. Trans.*, p. 383, Table I.

[2] Ib. p. 55, Table III.

of 1874 and 1882, an error of 1″ in the measurement would, for the maximum displacement, give an error of only 0″·185 in the deduced solar parallax.

'Moreover, it may be observed that in photographic records it is by no means important to catch exactly the phases of contact, as two photographs obtained at a sufficient interval afford the means of calculating to a great degree of refinement, and of tracing, the path of the planet, which, for the conditions of the problem, may be considered to be a straight line between the two positions recorded.

'Nor is it in any way essential, as it is with eye-observations, that favourable conditions should exist for retarding the period of contact at one station and accelerating it at another, because the chords representing the planet's path can be derived from photographic records with as much accuracy under what would be considered unfavourable conditions as under favourable conditions for eye-observations, for the length of the chords need not be directly considered in determining the nearest approach of the sun's and planet's centres.

'During the duration of the transit, it would be possible, in a clear state of the atmosphere, to obtain a series of photographs at intervals of two or three minutes, and any or all of these would be available for comparison with the records obtained at all the stations selected.

'The epoch of each photographic record is determinable with the utmost accuracy: 1st, because the time of exposure is not more than the $\frac{1}{50}$th or the $\frac{1}{100}$th of a second; and 2nd, because the instantaneous slide, as it flashes before the secondary lens, affords an audible signal[1] by striking against a stop a small fraction of a second after it has shut off the image of the sun. This interval might be determined by experiment and taken into account.

'In the Kew Photoheliograph the solar disc would, at the epoch of the transit of 1874, have a semi-diameter of 1965·8-thousandths of an inch (a diameter of nearly four inches), Venus a semi-diameter of 63·33 of these units, and the parallax of Venus referred to the sun would be represented by 47·85 of these units; the maximum possible displacement being 95·7 units, or nearly $\frac{1}{10}$th of an inch. In 1882 the sun's semi-diameter would be 1964·9 units; that of Venus 63·31 units; the parallax of Venus referred to the sun 47·82 units; the maximum possible displacement 95·6 units.

'When the photographs have been secured, the measurements by means of the micrometer, which would have to be performed, consist in determinations of the sun's semi-diameter, in units of the arbitrary scale of thousandths of an inch, the angle of position of different positions of the centre of Venus, and the corresponding distances of her centre from the centre of the sun. 'The measurements by means of the micrometer described in the "Phil. Trans." 1862, pp. 373–374, can be obtained to the $\frac{1}{2000}$th of an inch ($0''\cdot25$). and the position angles to one or two seconds

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[1] *Phil. Trans.*, p. 364.

of arc. For each photograph measurements made at different times are remarkably accordant; the greatest difference between the semi-diameter of the sun of the several eclipse pictures of 1860 was $\frac{9}{1000}$ths of an inch, or about 4″·5; but, on taking the mean of measurements of forty-five photographs by two different methods, the difference was only $\frac{1 \cdot 5}{1000}$ths, or about 0″·75. I am inclined to believe that the distance could be ascertained to within 1″ by means of a few pictures, and possibly to 0″·25, if a sufficient number of photographs were obtained.

'Fears have been expressed that the collodion, in drying, becomes distorted; experiments, however, in 1860–61 have demonstrated that the shrinkage is only in the direction of the thickness. But as, in the case of the solar parallax, no refinement of correction ought to be neglected, it would be quite easy to ascertain once more whether any distortion does take place, by taking photographs on glass plates on which lines about a quarter of an inch distant had been previously etched; the collodion, which should be rendered purposely contaminated with particles in suspension, should be poured on the ruled sides to avoid parallax. After all the operations of photography, the film would have to be examined from the back, and the position of certain impurities with reference to the ruled lines noted whilst the collodion was wet, and after it had dried.'

Three months later, Col. Tennant wrote a paper commenting on the practical details of Dr. De La

Rue's plan, in the course of which he pointed out that 'there is an advantage in this mode of observing the transit of Venus to which Dr. De La Rue has not alluded.[1] If accurate micrometrical observations can be made by means of photographic pictures, then the range of suitable stations can be greatly enlarged; for any two stations, 140° or 150° apart, can have the observations combined by choosing a suitable time, and, of course, by means of equations of condition, the observations of stations in all sorts of places could be used with their proper weight.'

To this Dr. De La Rue replied as follows:—

'With respect to the localities to be selected, the employment of the photographic method of observing transits of Venus has, as I have already stated, the advantage of rendering us independent of conditions favourable or indeed essential for eye-observations; for a few photographs obtained at each station would afford the means of ascertaining the path of the planet and its position at any given moment; and hence the proposal to determine the position of the planet's centre in relation to the sun's centre really includes the particular case contemplated by Major Tennant in the foot-note appended to his paper; but it is possible

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[1] 'Dr. De La Rue has pointed out,' says Col. Tennant, 'the facility with which the nearest approach can be got from the photographs at any one station, but if photographs be taken at two stations at a time when Venus is in the plane which includes them both as well as the earth's centre, these will show the whole effect of parallax; and it is the positions at these instants, and not the nearest approaches to the sun's centre, which should be compared.'

that the sun might be obscured at the critical time, at one of the two stations selected.'

In December 1869 I read before the Astronomical Society a paper, from which the following passages are extracts:—

'It is impossible to read Dr. De La Rue's account of the results of careful measurement applied to photographs of the solar eclipses in 1860 and 1868 without recognising that we have in photography, as applied to the approaching transit of Venus, one of the most powerful available means of determining the sun's distance. Within the last few years, solar photography has made a progress which is very promising in regard to the future achievements of the science as an aid to exact astronomy. So that doubtless, in 1874, astronomers will apply photographic methods to the transits of that year with even greater success than we should now be prepared to anticipate. It has therefore seemed to me that the photographic observation of the coming transit merits at least as full a preliminary inquiry as either Halley's or Delisle's method of direct observation.

'The result of an inquiry directed to this end has led me to the conclusion that photographers of the approaching transit should adopt for their guidance considerations somewhat different from those which have hitherto been chiefly attended to.

'It is undoubtedly true, as Dr. De La Rue has pointed out, that the photographer of the transit can readily take a large number of pictures, and by combining these, can ascertain with great accuracy the path of Venus across the solar disc. And by comparing the paths thus deduced for different stations a satisfactory estimate can be formed of the solar parallax. I do not wish to suggest any departure from this course of procedure.

'On the other hand, it is undoubtedly true, as Major Tennant has remarked, that the greatest effect of parallax will be obtained, for any two stations, when both stations—the earth's centre and the centre of Venus—are in one and the same plane. So far as those two stations are concerned, his remark is just, that it is the position of Venus at the instant when the stations are so situated, and not the nearest approach of Venus to the sun's centre, which should be compared. And further, Dr. De La Rue's comment on this, to the effect that his method in reality includes Major Tennant's, is also correct. In fact, there can be no doubt that the position of Venus at the particular instant referred to by Major Tennant can be far more exactly ascertained by a reference to the complete path of Venus for each station than from any attempt to secure nearly simultaneous photographic records at stations far removed from each other.

'But it appears to me that the method I am about to suggest, according to which the whole question will be reduced to the determination of a parallactic displacement of Venus on a line through the centre of the sun's disc, is the one by which the fullest assistance will be obtained from photography; while a source of error, which has not hitherto been specially considered, will be practically eliminated.

'It must be remembered that in the comparison of photographic records, whether for the determination of the path of Venus across the sun's disc at a particular station, or for the comparison either of Venus's apparent position or of her path as seen from two different stations, the accuracy of the results will depend in part on the certainty with which two or more pictures may be brought into comparison by means of a fiducial line or set of lines. It seems certain that no method can be devised by which all chance of error from this source can be eliminated. The great point would, therefore, seem to be to render its effect as small as possible.

'Now, let us consider for a moment Major Tennant's proposition, as giving a convenient illustration of the effects of any error either in the position of the fiducial lines, or in bringing those belonging to two pictures into exact correspondence. Let fig. 42 represent the result of a comparison between two photographs of the

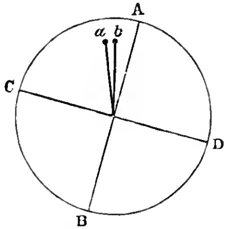

Fig. 42.—Illustrating the photographic and direct methods.

sun. A B and C D are fiducial cross-lines common to both pictures; *a* is the centre of Venus for one picture, *b* is her centre for the other; and on the exact measurement of *a b* depends the determination of the sun's parallax, so far at least as these two pictures are concerned. Now it is very obvious that if the lines A B, C D, for one picture, have not been brought into perfect correspondence with those belonging to the other, the distance *a b* will be correspondingly affected. In fact, it would appear that if the usual methods for making the correspondence as exact as possible are followed, almost as large an error would be introduced through this cause alone as by errors in the measurement of *a b*, since the two processes—the measurement of *a b* and the adjustment of the sets of cross-lines—depend on the very same circumstance, the nicety, namely, with which the eye and the judgment can estimate minute quantities of about the same relative dimensions.

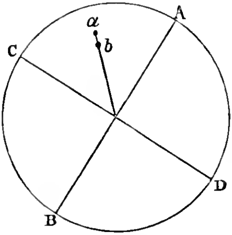

Fig. 43.—Illustrating the photographic and direct methods.

'But now, if *a* and *b*, in place of having the position shown in fig. 42, were situated as in fig. 43, it is clear that the distance *a b* will not be appreciably affected by any small error in the adjustment of the fiducial lines.

'The object, therefore, which it seems most desirable to secure is that Venus, as seen from two different stations at a particular instant, should have a relative parallactic displacement towards the sun's centre, or as nearly towards the sun's centre as possible. This amounts to adding to Major Tennant's conditions this further one, that the sun's centre should be in the same plane with the two stations—or rather to making this condition a substitute for that one which requires that the earth's centre should be in the same plane with the two stations. For, as a rule, we must not expect to be able so to arrange matters that two convenient stations on the earth, as well as the centres of the earth, Venus, and the sun, should be in the same plane.

'The object which Plate XVI. was originally intended to subserve was to determine what stations were most suitable for applying photography to the transit of 1874, on the principles above enunciated; though, as we have seen, the drawing illustrates also the character of the transit.'

In Chapter IV., in pp. 148–152, I have shown how all the chief elements of the transit could be deduced by considering the motion of Venus relatively to a pair of cones, each enveloping the sun and the earth, but one having its vertex outside the earth, the other having its vertex between the earth and the sun. In the paper from which I have been quoting, after explaining the construction of Plate XVI., I proceeded as follows:—

'We have only to invert fig. 35, and look at it from behind, to see what sort of path Venus would seem to traverse upon the sun's disc, either with reference to the earth's centre, or to any point of the earth's surface supposed to be properly depicted upon the small discs 1–15 in fig. 35.

'It follows, therefore, that if we want to determine two stations at which at any instant Venus would appear to have a relative parallactic displacement towards the sun's centre, all that is required is that we select two stations which are on the same radial line from the common centre of the circular sections in fig. 36.

'The positions of those radial lines which cross the earth's track *c d* are exhibited in Plate XVI.'

The paper went on to show that (from Plate XVI.) North India, Siberia, Cape Town, Kerguelen's Land, Crozet Island, and other places would be available for the application of the photographic method.

The American astronomers relied chiefly on the photographic method, *applied at stations where the whole transit can be seen.*

The plan they adopted for photographing the sun differed essentially from that which European astronomers propose to employ. 'For the purpose of obtaining an enlarged image on the photographic plate,' writes Professor Hilgard, of Washington (describing the ordinary method), 'the image of the sun, after being formed in the focus of the telescope, is enlarged by a lens or camera to the desired size, the photoheliographs, as they are called, being thus enlarged to a diameter of about four inches. This plan has been adopted for the photographic apparatus to be used by the British, German, and Russian parties commissioned to observe the transit of Venus. A different plan has, however, been adopted for the American parties, with the view of avoiding some difficulties to which the former method may be thought subject. These are conceived to reside in the fact that not only all imperfections in the focal image are thus enlarged, but that the optical imperfections of the camera are superadded. To avoid this objection it was deemed best to make the telescope so long that the image formed in its principal focus would need no further enlargement. Here another difficulty presented itself. The telescope must be forty feet in length in order to give an image four inches in diameter. Such a telescope, pointed at the sun, would scarcely be manageable. Hence the plan was devised, which Professor Winlock was the first to put into practical operation. It consists in fixing the long telescope in a horizontal position, and reflecting the sun's rays into the object-glass by means of a plane glass mirror, moved by clockwork, so as to throw the image of the sun continually into the telescope. This need not be done with great precision, since, as has already been said, the time of exposure is exceedingly small, and the mirror can at any time be adjusted. It is obvious that, in this arrangement, as much depends upon the perfect figure of the mirror, as in the other upon that of the enlarging lens; but it is, doubtless, an advantage that different methods should be employed, so long as a sufficient number of stations are occupied to give an independent result for the sun's distance from observations by each method alone, since such only can be considered as strictly comparable. This condition is amply fulfilled by the abundant provision made by the American Government for the observation of the important event in prospect.'

I may remark, however, that Professor Newcomb, with whom I had the pleasure of a conversation relative to the subject, attaches very great importance to the advantages of the American method. He remarked that by employing this method the astronomer is enabled to measure the distance of Venus from the sun's centre with an exact knowledge of the value of the deduced distance, because, the focal length of the telescope being known, the value of any distance indicated in the focal image is at once determined. All that is necessary, then, is to determine the centre of the solar image, which can be safely done by measurements made from the limb. Manifestly no photographic effects affecting the position of the limb in the photograph could appreciably affect the determination of the centre even though such effects were not absolutely uniform all round. But in the ordinary method of photographing the determination of the arc-distance of Venus from the centre is not reliable (in a problem of such extreme delicacy), because the *estimated* dimensions of the solar image could not be *accurately* determined, while the observed dimensions, being determined from the photographic limb of the sun, would be affected more or less by photographic irradiation. No apparent sharpness of the limb can render certain the fact that the limb in the photographic image corresponds to the true solar limb. I must confess that Professor Newcomb's reasoning seems to me irresistible. It will be observed that it does not depend on practical or technical knowledge of photography, since the photographic irradiation demonstrably exists, and is demonstrably variable in amount. In a conversation with Dr. H. Draper, of New York, whose experience in those matters is well known to be unsurpassed, I found Professor Newcomb's doubts fully confirmed. It is true that Dr. Rutherfurd, whose great practical experience in solar photography is unquestioned, agreeed with his eminent British rival in such work, Dr. De La Rue. But then it is to be remembered that both Rutherfurd and De La Rue viewed the matter as photographers, while Newcomb and Draper view it chiefly from an astronomical standpoint, and in this case the astronomical, not the photographic relations, were chiefly in question. Not handsome solar pictures, but pictures which could be confidently measured were wanted; and certainly the plan adopted by the American astronomers is that which best met this requirement.[1]

Experiments were made on the phenomena of contact by means of an artificial transit. Although the results obtained at Greenwich were not altogether so satisfactory as they might be, owing to the short distance from the observer at which the artificial discs have been placed, yet they only differ in degree, as presented in the following passage, from the more marked peculiarities observed at Washington (where I studied the phenomena myself):—

1. It requires considerable experience for an observer to appreciate all the definite changes of appearance which occur.

2. When two observers describe a particular phase which they see, and determine to observe this phase together, the times recorded by each are generally accordant within a fraction of a second.

3. The successive phases of an ingress or egress appear to follow each other sometimes rapidly, at other times gradually; so that in some cases all the phenomena are observed within three seconds, on other occasions the same series of phases is completed in ten seconds.

4. The time at which any particular phase is observed varies very slightly with the aperture of the telescope. When a telescope of good definition is

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[1] Lord Lindsay, it is to be noted, employed the same method as the American astronomers, after carefully testing with Mr. Ranyard, in a series of photographic experiments, the reliability of the two methods.

employed, the time of any phase at ingress is earlier than with an instrument of less perfect definition.

Observations such as these necessarily served to diminish the error of contact observations, especially as the observers not only measured the distances between the cusps as contact proceeded, but caused the phenomena of contact to be indicated photographically by an ingenious arrangement contrived by Janssen.

In considering the results obtained last December, it will be well to deal with the several methods separately, rather than to take the stations in order; in fact, no otherwise can clear or satisfactory ideas be obtained of the success of the whole plan of operations.

We may consider the methods in use as divided into three broad classes: the Delislean, the Halleyan, and the mid-transit method. This classification includes the photographic and heliometric operations, as well as the spectroscopic method for observing external contact; for both photography and heliometry may be regarded as means for determining the position of Venus at given instants, either near the beginning or end of transit, or near the middle of her chord of transit; and observations of external contacts, either at the beginning or end of transit, come under the head of Delislean operations, while the observation of the interval of time between the two external contacts is a Halleyan operation. But also the Halleyan and mid-transit methods can be considered together, because all good Halleyan stations are good stations for observing mid-transit. Let us take the classes in the order indicated, viz., first, the Delislean; secondly, the Halleyan. Speaking generally, it may be said that the English scheme was the only one which made direct provision for Delisle's method. The Americans declined, *totidem verbis,* to occupy any station where the whole transit could not be seen; the French refused to occupy the Mauritius or Suez (calling forth from English official astronomers an expression of strong dissatisfaction); and the Germans, though they occupied a station in Persia where the ingress could not be seen, yet specially provided for photographic work there during the middle of the transit. The Russians alone, having provided eleven Halleyan stations in Siberia, consented also to have observers at Delislean stations nearer to Russia in Europe, extending around E′ to the Black Sea, Caspian Sea, and Sea of Aral.

The only Delislean stations, properly so termed, near I, Plate VI., were those occupied by the three English parties, under Captain Tupman, in the Sandwich Isles, at Honolulu, Atooi, or Kauaii, and Owhyhee (or Hawaii). At the first two observations were successfully made, but at Hawaii, the least important, fortunately, of the three, the observers, Forbes and Barnacle, had bad weather.

Captain Tupman, at Honolulu, says that 'the sky was cloudless—a circumstance not altogether in our favour, as the heat of the sun was terrific.' At Waimea, Atooi, the weather was equally fine; 'not the faintest cloud or mist appeared.' On the whole, the observations made at the Sandwich Island stations were successful. Captain Tupman 'was not satisfied[1] with the determination of the moment when Venus had just completely entered upon the sun's face. A circumstance which appears to have taken many by surprise, though in reality it had been observed in previous transits, rendered the observation more difficult than it otherwise would have been. Venus has an atmosphere probably as dense as our earth's, and consequently there is a twilight-circle on Venus, and not only so, but the sun would be raised by the atmospheric refraction just as the setting sun with us is raised above the horizon after he has in reality (that is, in a geometrical sense) passed below it. The sun is raised at this time by more than his whole diameter. Now suppose Venus drawing near to the sun, and that we look at the point of her outline farthest from his. In so doing (and taking no account of the part of her atmosphere on her other side), we are looking at the sun in the same direction as an inhabitant of Venus stationed at that point we are looking at. But this being would see the sun close to his horizon, and raised as much as our sun is raised near the time of sunset (always supposing the atmosphere of Venus just like ours). The terrestrial observer is, as it were, behind the supposed inhabitant

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[1] The passage which follows is quoted from an article written by me for the 'Cornhill Magazine,' and appeared in the number for May, 1875.

of Venus, so that both see the same effect produced,[1] only the terrestrial being so far behind, the displacement of the sun is proportionately diminished. Nevertheless, he also would see the sun round that edge of Venus, even on our supposition that the near half of the atmosphere of Venus produced no effect. But in reality that half produces just the same effect as the other half, doubling the displacement, so that the observer on earth cannot fail to receive sunlight round that part of Venus, even, which is remotest from the sun. All along the edge of the half of Venus farthest from the sun his light is bent round and sent earthwards, though it need hardly be said that the result is to give only the finest possible thread of sunlight around that side of Venus, and no doubt, to ordinary observation, this thread would be imperceptible.[2] Now, the nearer Venus draws to the sun the brighter would this thread of light be, and when more than half of her disc had passed on to the sun's, the circle of light bounding the other half could hardly fail to be perceptible to a good observer armed with a powerful telescope. But then conceive the difficulty thus occasioned. What the observers had been specially instructed to look for (without, it would appear, the least hint of the peculiarity in question, though very

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[1] Much as though an insect were to look through a decanter of water at a page of print from a distance of a yard or so, while another looked in the same direction, but from a distance of two yards.

[2] Nevertheless, Prof. Newton, of Yale College, has seen the fine circle of light completely formed round Venus, during one of those passages of the sun which occur at intervals of about 584 days, but ordinarily carry her past him without transit.

carefully instructed about the quasi-mythical black drop) was the appearance of the sunlight between Venus and the sun, as her motion separated her from the sun's edge: but, on account of the action of Venus's atmosphere, a line of light (real sunlight, too) appeared round the part of Venus which would last cross the sun's edge, and became distinct before that part was even near true contact. Here, then, was the criterion of contact suddenly rendered useless, and the observer left to judge of contact in another way, if in the excitement of the moment he were not deceived by this thread of light so as to suppose it indicated that Venus had fully entered on the sun's face.' We find that Captain Tupman, though disconcerted, was not deceived, while Mr. Nichol, who observed with a smaller telescope, was deceived, but. apparently, not disconcerted. Mr. Nichol withdrew from observation thirty seconds before Captain Tupman, 'conceiving,' writes the latter, 'that contact was passed,' and recording nothing later. 'I am not at all surprised,' proceeds Captain Tupman, 'for there was nothing sudden to note, and the complete submergence' (here he regards Venus as sinking into the sun's disc) 'was so gradual, any one might have recorded ten seconds before I did, and have been quite as accurate. My first impression was, such an observation could not possess any value. It was something similar in principle to having to decide where the Zodiacal Light terminates; bearing in mind, of course, that we expected to get the contact within a second or so of time.

Unfortunately a photographic arrangement, by which it had been hoped that the true instant of contact would be indicated, was not successfully applied. This arrangement was what has been called the 'Janssen turning-wheel.' A circular photographic plate was so arranged that a series of sixty pictures could be obtained all round the edge, a second being given to each, so that the whole process would last one minute. If this minute were so taken as to include the moment of contact, then that moment would be known, because the successive pictures were all carefully timed. Now it would appear that Captain Tupman gave the signal at exactly the right time, and the atmospheric conditions were excellent; the turning-wheel was set going, and everything seemed to have worked well; but unfortunately, when the pictures were developed, it was found that the telescope had been wrongly directed, so that in every one of the sixty pictures 'the planet is cut in half.' This is the interpretation of the unpleasant telegram received from Honolulu, a few days after the transit, announcing that 'Janssen failed.'

At Port Possiet and Wladiwostock, near the western shores of the Sea of Japan, the ingress was well seen by Russian and American observers; but the acceleration there only amounted to about 6½ minutes, whereas, at the Sandwich Isles, it amounted to 11 minutes. At Yokohama and Nagasaki the Americans and the French (under Janssen) made good observations.

Accelerated ingress was fairly provided for; though at the best English stations for that phase Janssen's method failed, and the ingress was not very satisfactorily seen.

In the opposite region around I, Plate VI., there were three English observing parties, two in Kerguelen Island, and one on Rodriguez; while Lord Lindsay's station at the Mauritius, though not specially intended for Delislean operations, was even better placed than the Rodriguez station. I shall speak more at length of Lord Lindsay's operations further on. Here it is only necessary to say that his party missed ingress, though otherwise successful. At Rodriguez ingress was successfully observed, but as yet full details have not been published. It is known that the various parties stationed on Kerguelen Island were successful; but the nature of their success is not known, all the news yet received from that dismal island having been derived from the interchange of signals between the parties stationed there and a passing ship.

It would appear that a fair success has attended the employment of Delisle's method, as applied to ingress. At least, whatever defects may appear hereafter in the results by this method will be due to the inherent defects of the method itself, requiring as it does the observation of contact when the sun is not far from the horizon. Janssen's contrivance, by which it was hoped that this difficulty would be removed, failed entirely for ingress.

Turning next to egress, we have first to consider the operations around the point E′, Plate VI., where accelerated egress was to be observed. The stations provided by England for observing this phase were in New Zealand, though the Government Observatory at Melbourne, and the Observatory at Sydney, were fairly placed. It must be remembered, however, that everything depends on securing observations at both ends of either Delislean base-line by similar methods and by observers similarly trained. The only stations thus provided for were those in New Zealand; and unfortunately bad weather prevented the observation of egress at any of these stations. The American party, stationed at Queenstown, in Otago, were able to observe and photograph all the transit except egress, but the English parties were not even favoured with this partial success—or rather, I should say, with a degree of success which, in their case, would have been but partial; for to the Americans the observation of egress was a matter of small importance. It is impossible not to sympathise with Major Palmer, on account of this unfortunate end to a campaign for which he had made very complete arrangements. As a member of his party, writing from Wellington, remarks, 'it certainly seemed not too much to expect that, towards evening of a summer day in December, one of the finest months in the year (in New Zealand), an hour's clear sunshine might be vouchsafed to at least a considerable part of a colony somewhat larger than Great Britain, and famed for the beauty of its climate. Unhappily, these hopes and expectations were crushed by a day of pitiless weather. All through the 8th, and till mid-day on the 9th, from every quarter of both islands, telegrams conveying the same dismal tidings of mist and rain, cloud, gloom, and falling barometers, poured in on Major Palmer, the English chief, warning him that unless some sudden and unlooked-for change should very soon take place, the careful plans to which he and his colleagues had for so long given their time and energies would prove to have been made in vain. No change came, and failure was the result.' 'To crown their trouble, the day after the transit was provokingly, almost mockingly, fine. Some excellent sun-photographs, which were taken at Burnham on that day, showed how carefully the dry plates had been prepared, and how successful this, the least certain branch of the work, would have been. That the choice of stations was judicious, and that all was done that could be done with the means at command, is the opinion expressed everywhere.' This opinion will certainly be shared in England also: nor will astronomers be slow to accord to Major Palmer their fullest sympathy for his misfortune.

The Germans at the Auckland Islands, a station superior in value to any in New Zealand, achieved great success. Ingress, indeed, was obscured, 'but ten minutes later the sun shone out, and, the sky remaining clear to the end of the transit, some 150 photographs, as well as the observation of egress, were obtained.' The French at Caledonia Island saw everything except the egress. The French at Campbell Island had bad weather, as had also the Americans at Chatham Island. On the whole, egress was not well provided for.

Turning next to the region around E, Plate VI., where retarded egress was to be observed, we have to consider results of a mixed character. At all the best stations, viz., those occupied by the Russians, bad weather prevailed. The Russian Delislean observing parties were spread over Western Siberia, and included such important stations as Erivan, Tiflis, Taschkent, Astrakhan, Ornsk, Blagowestchensk, &c. At some of these stations the retardation would have been fully twelve minutes, whereas, at the English stations in Egypt and North India, the retardation amounted only to ten minutes. At Ispahan, where a German party was stationed, there would have been a retardation of $10\frac{1}{4}$ minutes; and as the results would have been directly comparable with those obtained at the Auckland Isles, a very valuable Delislean success would have been obtained had good weather prevailed at Ispahan. Unfortunately, though some good mid-transit photographs were secured, the contact at egress was missed through clouds. A Russian party, but not provided with first-class instruments, were successful at Teheran. But the chief success, so far as retarded egress was concerned, was obtained in Egypt and in North India: a circumstance which makes the ill-success of Palmer in New Zealand the more unfortunate, as it was with his observations that the Egyptian results were to have been compared. In Egypt thick haze prevailed on the important morning, until within a few minutes of the moment of contact. Then, however, the sun passed clear of the low-lying haze, and the contact was well observed. It was at first announced that Captain Abney had succeeded in getting a photograph of the contact by the Janssen instrument, but this news turned out to be incorrect. He just missed the actual contact, though it would appear that the moment of contact can be approximately determined from the photographs.[1] The German observers stationed in Egypt also made very satisfactory observations. At Roorkee, in North India, egress was well observed (as also the whole transit).

On the whole, the Delislean observations for egress have been fairly provided for. The special English plans have been defeated by bad weather in New Zealand; but the observations at Melbourne and Sydney will combine tolerably well with those made in Egypt and at Roorkee. The German observations in the Auckland Isles will combine admirably with the German observations in Egypt.

Before passing to the operations by other methods, it may be well to consider the general Delislean results, and the lessons they teach us as to future operations.

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[1] It seems tolerably clear that, in future applications of the Janssen arrangement, provision must be made for a longer interval than one minute.

It is, in the first place, certain, that if only Delislean operations had been provided for, as in the original programme, the measure of success achieved would have been very far from satisfactory. Early ingress was not, on the whole, well observed, and Janssen's method failed, so that the complete ingress operations are to some degree imperfect. The English special operations for observing egress have completely broken down through the failure in New Zealand. The success of the Germans is a strong point, but one success counts for little in a process where everything depends on reducing the final error through the *number* of successful observations. It is to be noted that the German success results entirely from the fact that the Auckland Isles were occupied. These are among the island groups to which I directed attention in 1869. At another of these, Chatham Island, the Americans had bad weather. At two others, St. Paul's Island and Campbell Island, the French had divided success, being fortunate at the former and unfortunate at the latter. All of these were specially named by me as suitable for applying the Delislean method, and also for observing the whole transit. Their occupation by America, France, and Germany, and the success of the Germans and French, sufficiently justify my suggestions, and more than meet the comments made on these suggestions by official persons. On the other hand, the partial failure of the English Government operations in no sense supports my position, except in so far as to justify the anxiety I expressed. Bad weather might equally have thwarted those arrangements proposed by me, which other nations carried out.

Turning now to Halleyan and mid-transit operations, we have a series of excellent results to consider.

(In the first place, let us examine the northern successes. At Nertschinsk, where the duration fell short of the mean by fully 15½ minutes, the Russians observed the whole transit with excellent telescopes, and obtained a number of measures and photographs. The success is particularly gratifying, as Nertschinsk was the best of all stations for applying Halley's method.) So far back as 1869 I called special attention to this point. But we have seen that even so late as February, 1873, the Astronomer Royal declined to believe that observations would be made there. 'Nertschinsk,' he said, 'is a station in high latitude, nearly 1,000 miles from the nearest sea. I presume that its climate is truly continental. At St. Petersburg, in the winter, the sun sometimes is not seen for several weeks together. I suppose that the same may happen at Nertschinsk. I doubt greatly the probability that any observations can be made there.' This he assigned as the sole reason why England should not occupy Southern Halleyan stations. Though four years had passed since I pointed to Nertschinsk as a suitable northern station, the fact remained still unknown to the person principally responsible for the English arrangements that *eighty per cent.* of winter days are clear at Nertschinsk. Of course Russia occupied this excellent station. She also occupied ten others in the region extending thence to the Sea of Japan, obtaining more or less complete success at six of them. The Americans were successful at Wladiwostock and Possiet, the Germans at Chefoo, the French in Japan and at Pekin. The duration was not, indeed, secured at all the Northern Halleyan stations, though it was at most of them; but where either ingress or egress was missed, the position of the chord of transit was effectually secured by mid-transit photographs and heliometric measurements. At Roorkee, in the long-neglected Indian region, the whole transit was observed and photographed under Col. Tennant's skilful supervision. The Germans photographed mid-transit at Ispahan, the Russians at Teheran. The whole transit was also observed by amateur astronomers at Kurrachee, Indore, and Calcutta, a fact rather showing what ought to have been done by official astronomers in England to strengthen the north Indian position, than (in all probability) adding much to the value of northern Halleyan operations.

In the Southern Hemisphere corresponding successes have been already reported, though as yet we have not received full accounts from some of the best southern stations, nor have we sufficient news of the nature of the success known to have been achieved at Kerguelen Island. But the Germans were successful in securing mid-transit photographs in the Auckland Isles, and the Americans at Otago and in Tasmania. At Sydney, Melbourne, and Adelaide, the chord of transit had been well secured. At Melbourne, in particular, the observations may be regarded as presumably most valuable, on account of the Government Observatory there. From the French stations in St. Paul's Island we have news, as already stated, of complete success; while at Campbell Island, unfortunately bad weather prevailed. At Rodriguez the whole transit was seen, and mid-transit well photographed. The French also secured more than two hundred daguerreotypes at Caledonia Island, and these will prove exceedingly useful.

But the most complete mid-transit success was that achieved by Lord Lindsay's party at the Mauritius, where photographic arrangements much more satisfactory than those made at the Government stations were combined with heliometric observations of mid-transit and spectroscopic observations of exterior contact, neither of which methods had been provided for at all by our official astronomers.[1]

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[1] It seems to me that Lord Lindsay's recent work in the cause of science is worthy of more than ordinary recognition. It is no new thing, indeed, for men of wealth and leisure to devote large sums to the advancement of science, though even in this respect what Lord Lindsay has done is noteworthy, seeing that the expedition fitted out by him will involve, first and last, a cost more than the entire amount devoted to the British Government expeditions. The remarkable feature in the present case is the personal activity displayed in the cause of science by one who might well have been content with the contribution of some seventeen thousand pounds to a single scientific expedition. Moreover, it is to be remembered that this was not by any means the first of Lord Lindsay's services to science. On the occasion of the eclipse of December 1870, Lord Lindsay fitted out an expedition at his own expense, all the preparations being much more complete than those made for either of our Government expeditions. On that occasion he accompanied his party, and took an important share in the work of observation. The next great astronomical event in which he

On the morning of the transit, at Mauritius, the sun rose concealed by clouds. Minute after minute passed without any sign of a break in the cloud-bank behind which the sun lay hidden. The important period of ingress, from first external contact to first internal contact, passed without a single opportunity of observing the motions of Venus as she entered on the sun's face. Had Lord Lindsay's party been relying upon the views entertained by astronomers five years ago, according to which only the Delislean

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assisted was the Total Eclipse of December 1871. On that occasion he fitted out an expedition at great expense, the results obtained by which were the most important ever obtained in eclipse observation, seeing that Lord Lindsay's party stationed at Baikul secured a series of photographs of the corona which finally established the solar nature of that wonderful object. Lord Lindsay did not himself join the expedition to India, but he took a large part in the work of analysing the results then obtained. A laboratory was fitted up by him in London, where, in company with Mr. Davis (to whose skill the success of the operations was in large part due) and other skilful assistants, he investigated carefully all the details of the photographs obtained in India. Even at that time preparations were being made and experiments carried on for the expedition to observe the transit at the Mauritius. Lieut. Gill was placed as superintendent over Lord Lindsay's observatory at Dunecht, where all the instruments and methods to be employed during the transit were carefully tested. At the laboratory in London experiments were made by Lord Lindsay and Mr. Ranyard on the peculiarities of solar photographs, in order that the best method of photographing the progress of the transit might be adopted. Lord Lindsay not only took a share in such work, and in superintending the preparations at Dunecht, but visited continental astronomers, physicists, and instrument-makers, inquiring into the qualities of various methods, processes, and instruments, in order that every available means for securing the best results might be employed. Lastly, he was not content to send out the expedition thus carefully provided for, but himself went to the Mauritius and shared in the work of observation.

method could be applied, their whole scheme of operations would have already failed, since the egress at Mauritius was not worth observing for that method. But the failure thus far signified only that one method out of three which the party had hoped to apply had failed them. We may say, indeed, that Halley's method had also failed, seeing that the duration of the transit could not now be determined; but the essential object in Halley's method is to determine the position of the chord of transit, and it still remained possible that this end might be secured. Fortunately the clouds which had so long concealed the sun cleared partly away about one hour after transit began, and, though the sun was visible only for a few minutes, photographs and measures were obtained. It was not till eight o'clock in the morning that it became fairly fine, and from that time the course of observation steadily proceeded until the end of the transit.

Lord Lindsay's place was in the photographic room. 'I took,' he says, '271 plates, out of which number perhaps 100 will be of value: cloud and the high temperature of the camera were much against me, the temperature varying from 96° to 100°. With the heliometer Mr. Gill obtained five complete determinations of greatest and least distance of the centres of the sun and Venus, besides nine measures of cusps, and two determinations of the diameter of Venus near the end of the transit. Dr. Copeland obtained fifteen measures of least distance of Venus from the sun's limb, and ten measures of cusps. The last internal contact was observed successfully, as also the last external contact.

I come next to a point which I would willingly pass over, but that the history of the transit would be incomplete without some account of it. It had been shown by me, in 1869, that Cape Town would be an excellent station for observing the middle of the transit. A letter published in the 'Times,' for February 15, from Mr. Dunkin, informed astronomers that the egress of Venus was satisfactorily observed at Cape Town by Mr. Stone, Astronomer Royal at the Cape. The satisfactory observation of egress at Cape Town unfortunately counts for very little more than the satisfactory observation of a sun-spot on the morning of the transit. The valuable stations for egress were those already considered, where egress was either notably accelerated near E′, Plate VI., or notably retarded near E. Cape Town is remote from either region, and observations of egress there had scarcely any value whatever. But Cape Town as a southern mid-transit station was *better than any station occupied either by our own country or by others.* Mid-transit photographs secured there, especially with all the advantages of a well-provided national observatory, would have been invaluable: so, also, would have been heliometric measures of Venus at the time of mid-transit, and afterwards until the end of the transit. It is now known that absolutely no provision whatever had been made for these most important purposes, Mr. Stone not possessing even the roughest appliances for observing mid-transit.[1] If the opportunity of utilising the Cape Town Observatory for mid-transit observations had simply been overlooked, little need have been said. That would have been no novelty, unfortunately. But special attention had been directed to the value of the station. In reply to a letter by Admiral Sir H. Cooper Key, inquiring whether Sir G. Airy had made arrangements for photographing mid-transit, the answer came that the method had been amply provided for; yet at the very best station for the purpose no provision had been made, though the station was exceptionally suitable, because of the Government Observatory there, and the presence of skilled astronomers. This would have been unfortunate as a mere case of negligence, but in its real aspect the matter is much more serious. It will not readily be forgotten.

In summing up the results of Halleyan and mid-transit operations, we must distinguish between contact observations, photographic results, and heliometric measurements. We must also draw a distinction between the various modes of photographing the transit employed at different stations.

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[1] It is impossible not to connect this with what happened in the case of the important total solar eclipse of April 1874, when totality lasted more than four minutes. On that occasion, though the track of total shadow passed close by our Cape Colony, Mr. Stone received no assistance whatever towards the proper observation of the eclipse. He had not even an equatorial telescope; but was obliged to observe with an altazimuth telescope 'borrowed from Mr. H. Solomon,' to which Mr. Stone attached a spectroscope with 'wrappers of wash-leather,' for want of more suitable appliances.

It seems probable that in future transits less reliance will be placed on contact observations than on photographic work. It is true that the results obtained during the recent transit show that the phenomenon of the 'black drop,' which in 1761 and 1769 occasioned so much trouble, depends on instrumental imperfections and atmospheric disturbances,[1] and can be practically eliminated by employing good telescopes and choosing stations where the sun will not be too low at the time of either internal contact. Nevertheless, a 'personal equation' comes in, depending on the fact that the eye itself is part of the optical arrangement for observing contact, and thus the observed moment of contact depends—to the extent of three or four seconds at least—on the observer's idiosyncrasies. This equation may be estimated by practice on models; but it must always remain doubtful how far, in the excitement of transit observation, the personal equation remains the same as in the calm of 'model transit' operations.

Heliometric measurements, again, seem inferior to the instantaneous work of photography. On any reasonable assumption as to the skill of the observer, it is impossible to believe that he can measure the position

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[1] Mr. Stone's ideas on this subject, on which so much stress was laid in 1868, have been entirely overthrown by the recent transit observations. Of all whose results have reached us, Mr. Stone himself was the only observer of skill who with a good telescope, saw any approach to the 'black drop' required by his theory. Certainly he saw and pictured what accorded most perfectly with his own ideas; but that only shows how likely preconceived opinions are to make the observer fancy he sees what he thinks he ought to see.

of Venus with an accuracy comparable to that with which the photographic picture can be measured, if only that picture is clearly defined, and not affected by imperfections such as would render the process of measurement uncertain. For instance, specks and stains on the photograph do no harm. A contraction of the film in photographs on glass would be mischievous, as also would be the effects of so-called photographic irradiation, if measurement depended on the size of the photographic image of the sun as affording a scale of measurement, while of course any optical defects would be fatal. But if such sources of error as these last can be in any way avoided, then photography must take its rank as *facile princeps* among the available methods for dealing with transits of Venus.

We must then, in considering the photographic results, attach chief—if not sole—value to the successes obtained by the Americans and Lord Lindsay using the long focal method, and by the French using Daguerre's process. Fortunately a sufficient number of results have been obtained by both methods, and in both hemispheres, to ensure the determination of the solar parallax to a much greater degree of accuracy than heretofore.

It is not too much to hope that the sun's distance may now be ascertained within limits of error not exceeding 100,000 miles on either side of the true distance.

On the whole, Science has every reason to be congratulated on the results achieved during the observation of the late transit. There were errors at the outset, and there were some points to be regretted in the final arrangements, but the more important mistakes were corrected in good time. In portioning to the different nations the honours due to them, we must assign to some countries special credit for some matters, while in other matters other countries took the lead. America devoted a larger sum to the expenses than any two nations together, and adopted excellent arrangements. Russia provided for the greatest number of stations; in fact, far more than England, America, and France together. Germany alone combined photographic and heliometric operations in both hemispheres. France distinguished herself by occupying the greatest number of difficult island stations in the southern seas. Lastly, to England must be assigned whatever credit is due to the first discussion of the subject, and the promulgation of a programme for the whole scientific world: had this programme been but correct, and had other nations only accepted the parts assigned to them, England would have been as easily first as she was in 1769, and as she may be—who knows?—in 1882.

As regards the transit of 1882, it may be hoped that what has happened in the case of the transit of the present year will serve in some sense as a warning to astronomers not to place implicit reliance on the opinions of any astronomer, however deservedly eminent, and also to prevent any unduly hasty expression of opinion by persons whose official position would cause the admission of error to be unpleasant. Adding to this consideration the fact that a large amount of practical experience in the value of the various methods of observation has probably been acquired during the transit of 1874, we may well hope that in 1882 even more valuable observations will be made. It needs but a short study of the sun-views forming Plates XIV. and XV. (or, preferably in some respects, figs. 37 and 38), and of the stereographic projection of Plate VII., to see that American astronomers will have to take by far the most important share in the work of observation in the northern hemisphere. Let it be permitted to us, however, to hope that England, by well-considered expeditions to southern stations, will remove any doubts that other nations may have entertained as to her zeal for science.

I may note here that while Halley's method fails totally for the transit of 1882, a method which, in some degree, would take its place may be applied with considerable advantage if stations in Patagonia, Tierra del Fuego, the Falkland Isles, or, better still, the sub-Antarctic islands directly south of Cape Horn, could be reached. I refer to a method which may be called the mid-transit method, and which consists in the determination (preferably by photography) of the distance of Venus from the sun, near the middle of the transit, at two stations where the difference of her distance from the sun's centre will be the greatest possible. It will be manifest to the student, if he considers what I have said in pp. 209, 210, that the advantages claimed for stations on a radial line through the centre of Venus's shadow-cone (O in fig. 36) culminate when the earth is near the centre of her chord of passage through the shadow-section V V′. Wherever convenient stations exist for advantageously photographing the whole chord of transit, it would of course be absurd to select a station only advantageous

Fig. 44.—Illustrating the Mid-Transit Method (Transit of 1882).

for the middle of the chord; but in the transit of 1882 the whole chord cannot be very advantageously photographed at southern stations; and there will be a decided advantage in securing mid-transit photographs (as well, of course, as photographs of the beginning and end, and series of photographs for the whole transit where practicable).

Fig. 44 illustrates the conditions under which this method would be applied in 1882. The face of the earth here shown is that turned sunwards at the time of mid-transit. The parallel lines show where the displacement *from* and *towards* the sun's centre (in northern and southern regions respectively) equals 1, 2, 3, . . . 8, 9, tenths of the maximum displacement at M and N respectively. The circles indicate the solar elevation. The shaded lunes include all regions where the displacement is not less than half the maximum, and the sun not less than 150° above the horizon. Unfortunately, the southern lune is a region singularly wanting in convenient stations. Possession Island and Victoria Land occupy the very best position possible.

It is not probable, judging from what Sir G. C. Lewis has shown respecting centenarians, that any of my readers will witness the transits of 2004 or 2012. Nevertheless, it may be interesting to know the circumstances of those transits and the regions of the earth where they will be wholly or partially visible. Mr. Hind, Superintendent of the 'Nautical Almanac,' has calculated the elements of the two transits of the beginning of the twenty-first century. His results are thus presented in the 'Notices' of the Royal Astronomical Society for February 1872, p. 184 :—[1]

Transit of 2004.

Greenwich Mean Time of Conjunction in Right Ascension = June $7^d\ 20^h\ 51^m\ 28^s{\cdot}8$.

For the centre of the Earth.

	d	h	m	s		°
First external contact	June 7	17	3	43	at	115·0
First internal contact	,,	17	22	35	,,	118·0
Second internal contact	,,	23	5	40	,,	214·6
Second external contact	,,	23	24	32	,,	218·5

The angles are reckoned from N. towards E. for the direct image.

At Greenwich the entire transit will be visible.

Transit of 2012.

Greenwich Mean Time of Conjunction in Right Ascension = June $5^d\ 13^h\ 4^m\ 44^s{\cdot}3$.

For the centre of the Earth.

	d	h	m	s		°
First external contact	June 5	10	22	11	at	40·3
First internal contact	,,	10	39	56	,,	37·8
Second internal contact	,,	16	42	6	,,	293·1
Second external contact	,,	17	0	0	,,	290·5

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[1] M. Leverrier's Tables of the sun and Venus represent so closely the motions of the earth and Venus in their orbits, that there can be little reason to doubt that the Tables will be as sensibly perfect in 2004 as they are at the present time.

At Greenwich the egress only will be visible, the sun rising at $15^h\ 46^m$.

These results are illustrated by the projections forming Plates VIII. and IX., and by the transit chords shown in Plate I. (the Frontispiece). The reader who has followed the explanations of Plates IV. and V. will have no difficulty in understanding the plates illustrating the transits of 2004 and 2012.

We cannot doubt that when the transits of 2004 and 2012 are approaching, astronomers will look back with interest on the operations conducted during the present 'transit-season;' and although in those times in all probability the determination of the sun's distance by other methods—by studying the moon's motions, by measuring the flight of light, by estimating the planets' weight from their mutual perturbations, and so on, will far surpass in accuracy those now obtained by such methods, yet we may reasonably believe that great weight will even then be attached to the determinations obtained during the transits of the present century. The astronomers of the first years of the twenty-first century, looking back over the long transitless period which will then have passed, will understand the anxiety of astronomers in our own time to utilise to the full whatever opportunities the coming transits may afford; and I venture to hope that should there then be found, among old volumes on their book-stalls, the essays and charts by which I have endeavoured to aid in securing that end (perhaps even this little book in which I record the history of the matter), they will not be disposed to judge over-harshly what some in our own day may have regarded as an excess of zeal.

TRANSIT OF 1874.

Table I.—*Places where Ingress was accelerated.*

Station	Sun's elevation	Acceleration in minutes
	deg.	m.
Woahoo . .	19·8	11·2
Hawaii . . .	19·7	11·1
Aitou Id., Aleutian	10·8	10·3
Marquesas Island .	17·7	7·9
Mouth of Amoor R.	14·0	7·6
Jeddo . . .	32·1	6·8
Otaheite . .	29·7	6·4
Nertchinsk . .	10·1	5·8
Tsitsikar . .	17·0	5·8
Kirin-Oula . .	19·5	5·7
Nagasaki . .	32·7	5·3
Tientsin . .	22·2	5·0
Pekin . . .	20·8	4·3
Shanghai . .	28·5	3·9
Nankin . . .	27·1	3·6
Canton . . .	35·5	1·6
Hongkong . .	36·2	1·6

Table II.—*Places where Ingress was retarded.*

Station	Sun's elevation	Retardation in minutes
	deg.	m.
Crozet Island .	15·0	12·6
Enderby Land .	20·0	11·8
Kerguelen Land .	27·5	11·6
Macdonald Island .	31·0	11·2
Kemp Island .	30·0	11·1
Bourbon Island .	12·4	11·1
Mauritius . .	14·1	10·7
Amsterdam Island	34·1	10·3
Rodriguez . .	19·0	9·9
Sabrina Land .	45·0	8·2
Adélie Land . .	45·0	6·8
South Victoria Ld.	38·5	6·0
Perth (Australia) .	65·0	5·3
Royal Co. Island .	62·0	4·5
Madras . .	21·0	4·0
Bombay . .	12·5	3·8
Macquarie Land .	52·0	3·5
Hobart Town .	67·0	2·8
Adelaide . .	75·0	2·5
Melbourne . .	75·0	2·2

TRANSIT OF 1874.

Table III.—*Places where Egress was accelerated.*

Station	Sun's elevation	Acceleration in minutes
	deg.	m.
South Victoria Ld. (Possession Ilnd.)	25·0	11·4
Adélie Land	34·0	10·6
Campbell Island	26·0	10·3
Emerald	30·0	10·3
Macquarie Island	32·0	9·8
Chatham Island	16·0	9·8
Canterbury (N.Z.)	22·5	9·3
Wellington	20·0	9·2
Sabrina Land	43·0	9·2
Enderby Land	39·0	8·5
Royal Co. Island	42·0	8·5
Auckland	19·2	8·5
Kemp Island	51·0	7·6
Hobart Town	40·0	7·6
Melbourne	43·0	6·6
Sydney	37·2	6·6
Adelaide	47·8	5·8
Kerguelen Land	57·1	5·0
Crozet Island	47·5	4·2
Perth (Australia)	66·2	3·6

Table IV.—*Places where Egress was retarded.*

Station	Sun's elevation	Retardation in minutes
	deg.	m.
Orsk	12·5	11·8
Omsk	11·5	11·7
Astracan	12·0	11·6
Aleppo	14·6	10·5
Peshawur	31·5	10·3
Alexandria	14·0	10·0
Suez	16·1	9·8
Nertchinsk	10·1	9·8
Delhi	38·0	9·4
Tsitsikar	12·0	8·7
Bombay	45·0	8·5
Pekin	21·0	8·6
Kiriu-Oula	14·0	8·4
Tientsin	17·1	8·4
Calcutta	45·3	8·2
Aden	30·0	7·8
Nankin	27·0	7·6
Madras	52·0	7·4
Shanghai	26·0	7·2
Canton	37·0	6·6
Hongkong	37·0	6·5

TABLE V.—*Difference in the Duration of the Transit of Venus in 1874, at 14 Northern and 13 Southern Stations.*

Northern Stations	Southern Stations												
	Kemp Island	Crozet Island	Macdonald Island	Kerguelen Land	Emerald Island	Macquarie Land	Campbell Island	Royal Co. Island	Auckland Island	Hobart Town	Canterbury (N.Z.) and Bourbon Island	Melbourne and Mauritius	Adelaide and Rodriguez
	m	m	m	m	m	m	m	m	m	m	m	m	m
Nertchinsk . .	34·2	32·4	32·3	32·2	29·3	29·1	28·7	28·6	28·0	26·0	25·7	24·4	23·9
Tsitsikar . .	32·9	31·1	31·0	30·9	28·0	27·8	27·4	27·3	26·7	24·7	24·4	23·1	22·6
Kirin-Oula .	32·7	30·9	30·8	30·7	27·8	27·6	27·2	27·1	26·5	24·5	24·2	22·9	22·4
Tientsin . .	32·0	30·2	30·1	30·0	27·1	26·9	26·5	26·4	25·8	23·8	23·5	22·2	21·5
Jeddo . .	31·8	30·0	29·9	29·8	26·9	26·7	26·3	26·2	25·6	23·6	23·3	22·0	21·5
Pekin . .	31·5	29·7	29·6	29·5	26·6	26·4	26·0	25·9	25·3	23·3	23·0	21·7	21·2
Tchefoo . .	31·0	29·2	29·1	29·0	26·1	25·9	25·5	25·4	24·8	22·8	22·5	21·2	20·7
Nagasaki . .	30·5	28·7	28·6	28·5	25·6	25·4	25·0	24·9	24·3	22·3	22·0	20·7	20·2
Bonin Is. . .	30·0	28·2	28·1	28·0	25·1	24·9	24·5	24·4	23·8	21·8	21·5	20·2	19·7
Nankin . .	29·8	28·0	27·9	27·8	24·9	24·7	24·3	24·2	23·6	21·6	21·3	20·0	19·5
Canton . .	26·8	25·0	24·9	24·8	21·9	21·7	21·3	21·2	20·6	18·6	18·3	17·0	16·5
Hongkong . .	26·7	24·9	24·8	24·7	21·8	21·6	21·2	21·1	20·5	18·5	18·2	16·9	16·4
Peshawur . .	26·6	24·8	24·7	24·6	21·7	21·5	21·1	21·0	20·4	18·4	18·1	16·8	16·3
Delhi . .	25·5	23·7	23·6	23·5	20·6	20·4	20·0	19·9	19·3	17·3	17·0	15·7	15·2

TABLE VI.

For determining the effect of changes in the value of the Sun's equatorial horizontal parallax (at his mean distance) on the estimated mean distance.

Parallax	Mean Distance (in miles)	Difference for 0″·01 (in miles)
8·0 ′	102,173,020	126,142
8·1	100,911,600	123,061
8·2	99,680,990	120.097
8·3	98,480,020	117.240
8·4	97,307,620	114,478
8·5	96,162,840	111,820
8·6	95,044,640	109,244
8·7	93,952,200	106,766
8·8	92,884,540	104,362
8·9	91.840,920	102 045
9·0	90,820.470	99,805
9·1	89,822,420	97,632
9·2	88,846,100	95.532
9·3	87,890,780	93.504
9·4	86,955.740	91,530
9·5	86,040.440	89,624
9·6	85,144.200	87.780
9·7	84,266,400	85,986
9·8	83.406,540	84.248
9·9	82,564,060	82,564
10·0	81,738,420	

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